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THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

AUGUST, 1852.

ART. I.—*A Case of Spinal Irritation, attended with loss of Vision; Recovery.* By CHARLES FISHBACK, M. D., of Shelbyville, Ind.

John Smith, æt. 30, temperament sanguineo-nervous, the latter predominating decidedly; florid complexion, light hair, blue eyes, brain large anteriorly and superiorly; health feeble in childhood, but generally good since puberty. At about five years of age was blind for several weeks, cause unknown, eyes were red, but not painful. The physician who was called in, said he could save the right eye, but not the left, by putting in it a powder. This caused great swelling, threatening the bursting of the globe, and violent pain. The sight of the left eye was restored, but the right was of no use until the winter of 1850 and '51, after the loss of vision of the left eye, when the function of the right eye gradually and steadily improved, until in July, 1851, the pain which had been

previously confined to the left side invaded the right, and then impairment of vision of right eye occurred, and progressed to complete loss early in November, 1851. He has a sister older than himself, who was blind for some time during her youth, but has not been since.

Mr. S. is a farmer by occupation; has been married four years, and has two children. In September, 1850, was attacked with pain of the left side of his head and face, extending exactly to the mesial line anteriorly and posteriorly, and embracing all of that side of the head and face excepting the globe of the eye. About the middle of November, 1850, having taken severe cold from getting wet, violent pain appeared in the globe of the left eye, and in a few days, vision of that eye became impaired, the globe assuming a more prominent appearance, and becoming, to his friends, larger than formerly but without redness; vision entirely lost in about two months. About the time of taking cold in November, Dr. McGeaughy was called in. Prescribed an active cathartic, to be followed by quinia and Ferri carb. in large doses, without sensible diminution of pain, and causing great irritability of stomach, attended with distressing flatulency and rejection of every article of food and drink. The quinia and iron were continued at intervals until May. Large portions of opium and morphia were given during this period, to allay the almost insupportable pain, inducing the ordinary unpleasant effects of opium in large and successive doses, in an aggravated degree. Dr. M. also gave guiac for rheumatism, which he said was a complication. There was pain of the shoulders and upper third or more of spinal column.

In January, 1851, there was a remission of the pain for a short time. In April, there was entire absence of pain for two or three weeks, and he went to work. During the latter part of winter and early in the spring, resort was had to electro-magnetism, sometimes with relief to pain, but not at others.

Towards the close of May, 1851, he visited Cincinnati, and consulted Prof. Mussey, whose written opinion and prescription are as follows, viz: "There is some reason to hope for relief from a suitable diet, with some medicine. Never let the bowels become costive. If there be a tendency that way, take at bed-time of rhubarb and charcoal, each 10 grs., and ginger 5 grs., mixed together; or take occasionally at bed-time, one of the following pills, viz: Ext. colocynth comp. ℈iiss, sapo. castil. ℈ss, oil Croton, gtt., j. m. ft. pills, 12. One hour after eating, take, in a tablespoonful of rain water, four drops of the solut. Fowleri mineralis; continue it for five or six weeks, unless the vision is very much improved in that time, or some of the specific effects of the article should manifest themselves. In either case, its use may be suspended. Use no tobacco in any form, nor strong drink, nor coffee. Avoid the eating of meat. Perhaps you may take a boiled egg once a day, if it be found to agree with the stomach. Drink weak black tea, or water. Eat no bread that is less than one day old. Never eat anything between meals."

There had been a tendency to constipation all along. No sensible effect was produced by the solution Fowleri mineralis, though taken according to directions. Only one or two of the pills was ever taken. There was comparative comfort from middle of May to middle of July, at which time pain attacked the right side of his head and the right eye. He now came under the care of Dr. Wolf, who removed six fangs, and ordered solution sulphate of magnesia to keep the bowels open, and pills containing a mercurial, which soon produced slight ptyalism, without any improvement, but on the contrary great prostration, so as to require aid in changing his place from bed to chair. He also gave, night and morning, a pill containing strychnine. He also ordered a resumption of the quinia and iron. No benefit followed, but general and complete prostration.

He came under my notice October, 1851, very weak, much reduced in flesh, pulse feeble, and irritable, and a little over 100 beats a minute. Tongue furred and deeply fissured in every direction over its entire surface. Bowels torpid, gastric flatulency great, and frequently joined with acidity. Appetite poor and irregular. No enlargement of abdominal viscera perceptible, nor tenderness, except of epigastrium, which is exceedingly tender; urine high colored, and variable in quantity. Excessive tenderness over the entire spinal column, a little greater in the upper two-thirds of the dorsal region than elsewhere; complains much of pain and weakness of the back. Pain of the right side of the head constant and severe, but variable in degree, being greatest when flatulency and acidity of the stomach prevailed. Has been troubled with almost icy coldness of feet since about the time that stomach became irritable, or nearly a year, and is most felt when pain of head is severest. For sometime past, cannot sleep more than two or three hours in twenty-four, and often less; sometimes none at all. Sense of fatigue throughout the muscular system, but no pain or tenderness. Pupils dilated almost to their full extent, but contracting considerably in a strong light. Cornea clear; conjunctival color not perceptibly altered. A very few tortuous vessels over the sclerotic portion of the globe. Neither the circumcorneal zone, nor the straight, and fine vessels indicative of sclerotitis visible with a good pocket magnifier. Globes prominent, with fullness of palpebræ, upper and lower; vision of the left eye entirely gone. Can see a little with right eye. Coldness of extremities, superior and inferior, general, intense, and almost constant.

Regarding the state of vision as dependent mainly upon disease of the optic nerve, or of that part of the brain from which it arises, and *this partially*, upon the state of the constitution, I directed treatment to the latter, First:

Kept the bowels in a soluble state, with pill colocynth, compos., of the United States Dispensatory, modified by the addition of gamboge and rhubarb. Quieted his nervous system and stomach, by scarified cups over spine and stomach, followed by emplast pix. Burgund.; hot pediluvia with mustard; nitro-muriatic acid to entire cutaneous surface, and iij grain pills of ext. hyosciamus, repeated every hour or two, until the desired effect is obtained. Endeavored to correct acidity of stomach with Bismuthis subnitrat., which was only partially successful, neutralizing, but not preventing acidity. During the month of October, his improvement was steady, and much more rapid than I had dared to hope for. Though not perfectly free from pain of the head, he was comparatively so. Sleeps sufficiently, and it is refreshing, and much of the time without the anodyne. Pulse became slower, about 80 per minute, stronger and calmer. Bowels soluble, and daily moved, without medicine after the first week. Tenderness of spine and epigastrium scarcely perceptible. Strength much increased; spirits revived and cheerful. Vision of right eye much improved, without sensible change in the appearance of the eye.

October 24th. Introduced a seton in the nape, and kept it running for about six weeks. A week or ten days after its introduction, he contracted a severe cold, from exposure during the greater part of a night, caring for a sick horse. An increase of the general malaise, and of the head in particular, followed. Ung. veratria to forehead, mastoids and nape; Ung. tart. antimony to epigastric and lumbar spinal regions, after scarified cups and pills argent. oxidi, grs. ij, ter die, November 12th, and subsequently, with benefit.

November 18th. Having suddenly risen from his seat, and stepped to the door, he fell to the floor, and was insensible and apparently breathless for some minutes; face pale. About an hour afterwards, he commenced

talking, as if he had been asleep, without recollection of anything after reaching the door. This I suppose to have been syncope. For a week afterwards the pain, coldness of extremities, and restlessness, were greatly increased, with loss of appetite. There were also occasional flushings of the face, with a sense of fullness about the head. With the renewal of pain, came diminution of vision, until in a few days, it was entirely lost.

He was at home nine miles from town on the 18th, and until the 28th. I visited him on the 22d, and found him suffering beyond expression. Ordered, ℞. syrup sarzae comp. (U. S. Dispensatory,) ℥j, potassii iodidi, grs. v, ter die. Caused slight nausea, but was continued. Pills ext. hyosciamus, and frictions to cutaneous surface, with hot pediluvia. He got better and came to town on the 28th. The next day, I applied one leech to the scalp near the anterior and inferior portion of the right parietal bone, and another over the occipital bone, about an inch above and behind the mastoid, of same side; at both of which points there was considerable tenderness. Ordered, ℞. magnesia sulphatis, ℥i, ant. tart. grs. i, Vin. colchici f. ℥ss; aquae ℥iv, M. S. a table spoonful sufficiently often to insure two or three dejections daily.

December 3d. Two leeches to anterior and one to posterior points of scalp as before.

December 5th. Improved feeling to-day, for the first time since about the 20th of November, distinguished day-light.

December 8th. Still further improved in feeling, appetite and strength—quite free from pain. Sleeps well without anodyne; bowels regularly open without medicine, some flatulence yet, but slight; can see persons moving about the room.

Feb'y 20th. He has steadily improved; can see to ride over the country, and to walk about town; as heavy as ever he was, and feels as well as he ever did.

Shelbyville, Ind., July, 1852.

ART. II.—*Two Cases of Fatal Injury of the Head.* By ROBERT DURRETT, M. D., Resident Graduate of the Louisville Marine Hospital, for 1851.

Miller in his Practice of Surgery says, that "between pure examples of concussion and compression of the brain, there is no difficulty in drawing a sufficiently broad distinction in practice as well as in theory. The one, a case of syncope; the other, one of coma. In concussion, the symptoms immediate; insensibility incomplete; the organs of special sense capable of being roused; the muscles contractile, and the limbs under strong stimulus, undergoing movement; the breathing soft and gentle; the pupils not uniformly dilated, though insensible to light; the pulse rapid, small, indistinct, perhaps for a time imperceptible; vomiting; no involuntary evacuations; the skin cold, pale, and shrunk. In compression, the symptoms not necessarily immediate; insensibility complete; the organs of special sense incapable of being roused; the muscles relaxed, paralyzed; the limbs motionless, until cessation of the state of compression, and advance of the inflammatory process; breathing labored, slow, and snoring; the pupils dilated and insensible; the pulse slow, distinct, perhaps full, sometimes intermittent; no (or seldom) vomiting; feces passed involuntarily; retention or dribbling urine; the skin warm, and often bedewed by perspiration."

The history of the two following cases does not accord with the above quotation.

John Convoy, aged thirty, a marine, was admitted into the surgical ward of Louisville Marine Hospital, March 23d, 1852. He gave the following very unsatisfactory account of himself, to wit: That he had been drinking for several days, and either fell and injured himself or received blows from an unknown source. Upon examination, found exquisite tenderness over the spine between the shoulders,

which was relieved by the application of wet cups. The patient complained a little of his head, tongue was good, pulse about 60, natural in volume, pupils susceptible to light. I supposed the patient to be laboring under the effects of a debauch. Ordered a purge.

March 24th. Patient still unable to tell how he was hurt, but thinks that he was struck by some one whilst drunk; seems to answer all questions propounded intelligibly, complains of constant and distressing head ache; no marks of external injury. Ordered patient's hair trimmed, and cold water applied to the head.

March 25th. Patient sleeps almost constantly, sings and talks in sleep; when aroused, which is done with difficulty, answers questions directly; pulse 70, pupils susceptible to light. Having heard to-day through friends of the patient that he had been struck by the mate of the boat several times, supposed the patient laboring under inflammation of the brain, the result of concussion caused by the infliction of the blows. Ordered submur. hydrar., grs. ij, every three hours, the continuation of cold water to the head.

March 26th. No perceptible change, except slight opisthotonos when patient attempts to move.

March 27th. Opisthotonos continues; patient walks across the ward to the privy.

March 28th. Opisthotonos continues to increase in severity, pulse less full, more frequent, skin dry, rather warm. Ordered, a blister to the back of head and neck.

March 29th. Stopped mercury; patient ptyalised; slight delirium, decided opisthotonos, pulse 100, intermitting; bowels moved by medicine.

March 30th. Patient died with the severest convulsion I have ever seen. On a post-mortem examination, found a fracture almost throughout the entire extent of the lambdoidal suture on the right side; between two and three ounces of coagulated blood immediately beneath the frac-

ture, between the dura mater and cranium; over an ounce of serum in the right ventricle; lymph at the base of the brain, and softening of the spinal cord. The patient's intellect remained unimpaired until within a few hours of his death, showing that the cerebellum is capable of sustaining the gravest injury without materially affecting the mind. It is very difficult, and in some cases almost impossible, to make out a correct diagnosis in injuries or diseases of the brain. Many of the most prominent symptoms of compression may be present during life, and yet autopsies reveal none of the pathological changes, which the symptoms are supposed to indicate, as in the following case.

Allen Smith was admitted into the Hospital, March 15, 1852, one hour after the reception of two incised, and one contused wound upon the head, which felled him insensible to the ground. The patient had retention of urine, dilated pupils, warm extremities, partial unconsciousness, involuntary fecal discharges and stertorous respiration, for several hours before death. A careful examination of his head did not enable us to account for his death, which occurred on the 17th of March, thirty-six hours after injury. We found considerable engorgement of the venous system, and some serum in the ventricles; but not more than we usually find in autopsies—not so much as we invariably find in persons who die of *mania a potu*. I give these particulars because Smith was a man of very intemperate habits, and at the time he received the blows was, without doubt, the subject of delirium tremens.

Louisville, 1852.

ART. III.—*A Case of Glanders in the Human Subject.* By JOSIAH T. WATTS, M. D., of Enterprise, Miss.

Axum Permenter, a young man about twenty seven years of age, stout and healthy, of rather intemperate

habits, had been for some time a teamster on a railroad, sleeping on the ground at night. Had a horse to die of the glanders, and in about ten days afterwards, began to complain of an unusual pain or fullness in the head, general lassitude and weakness of the whole system. Continued in this condition for three days without applying for medical aid. I saw him on the 23d of June, he complained of pain in his bowels, and had a thin bloody mucous discharge from the bowels. Believing it to be diarrhea, (for a good many of the family were suffering with this complaint at that time,) I gave him calomel and opium, conbined with kino. The diarrhea ceased under this course of treatment, and he began to improve for two days. He then complained of pain just over the right eye, with a headache; pulse full and bounding, tongue coated with a white fur; tip red, great difficulty in projecting it, the whole nervous system in a complete tremor. Bled him copiously from the right arm, applied a blister to the back of the neck, which discharged freely up to the time of his death. Ordered 10 grs. calomel, with 2 of ipecac at bed time; at 1 o'clock at night, gave ¼ gr. sulphate of morphine, under which he rested well.

24th. Next morning bowels opened twice; no fever, slight head ache, right eye swollen, and also the right nostril. Ordered during the day, quinine, nitre, and paregoric combined; skin became moist after the second dose; at night a Dover's powder. Patient rested pretty well.

25th. Pulse soft, tongue unchanged, bowels moved once; the right side of the face very much swollen, so much that he could not see out of the right eye; right nostril closed; a slight bloody yellow water issued from the eye and nose. Applied cloths wrung out of cold water medicated with acetas plumbi; gave morphine and calomel.

26th. Patient rested badly; face continues to swell; the left side swollen as much as the right; cannot see at

all; gets his breath through his mouth alone; a yellow discharge mixed with blood, from both his eyes and nostrils, removes the cuticle wherever it touches; has to be propped up in bed on account of the discharge from his nostrils, which is swallowed when he is in a recumbent posture; the nose dark colored, with small ulcers over it; difficulty of swallowing; in attempting to drink water about half runs back through the nostrils; the discharge is exceedingly offensive, and similar to that of glandered horses. Saw him again in the evening—condition as in the morning, except that his throat is much swollen; he can scarcely utter a single word, owing to the swelling of his lips; the discharge still continues from both his eyes and nostrils as before, but is not now sanguineous. Complains only of a difficulty in breathing; the discharge is extremely offensive; nose almost black; neck swelling very fast.

27th. Died at 8 o'clock, A. M., the discharge continuing from his nose and eyes until he died.

No post-mortem examination was made. Several persons present at the time of his death remarked, that the odor of the discharge was the same as that which they had often perceived about horses with glanders.

June 30, 1852.

ART. IV.—*On Vital Organic Chemistry.* By C. CALDWELL, M. D., Late Professor of the Institutes of Medicine in the University of Louisville.

[This article was intended as a Report to the American Medical Association, but its venerable author was unable to attend the meeting of the Association, and his views are submitted to the profession through the pages of this *Journal.*]—*Editors Western Journal of Medicine & Surgery.*

At a meeting of the American Medical Association, at

Cincinnati, May, 1850, the following resolution was adopted:

"*Resolved*, That Dr. Caldwell be requested to take into consideration the subject of *Vital Organic Chemistry*, and report to the next meeting, whether, in his judgment, it can be justly called a *branch of science*, and, if so, how far a knowledge of it can be rendered available to the welfare of man."

Were it not for one obstacle, the preparation of this Report would be a humble and comparatively an easy task. But that hindrance renders it complex a d difficult.

The obstacle alluded to is the difficulty of ascertaining a correct and satisfactory knowledge of the subject of the report, apprehending and definitely comprehending the precise import of the phrase "*Vital Organic Chemistry*."

This is not less than the twentieth—perhaps than the fiftieth time I have written, lectured, debated, or in some way exercised my mind in the consideration of the same subject. And on each repetition of the exercise, I have endeavored in vain to acquire a knowledge of the interpretation applied by the members of the chemico-physiological sect of the medical profession, to the expression "vital organic chemistry." For it is to that sect alone that the expression belongs. By the vital sect it is disowned as *an error*, or a *nullity*. To explain myself more fully on the subject.

In the year 1844, when engaged in a controversial correspondence on this topic, I proposed to my antagonist for solution, the following plain and pointed interrogatories:

"What is the genuine meaning of the phrases "*Animal Chemistry*," "*Vegetable Chemistry*" or "*Vital Organic Chemistry?*" For I presume the import of the three forms of expression is the same. The attribute of *vitality* must be involved in each of them. For vegetables and animals are equally *vital*.

"Do the advocates of the doctrines of animal, vegetable, and vital organic chemistry contend, that, in the *formation*

of living animal and vegetable tissues and secretions, the forces called *chemical affinities* or *attractions* and *repulsions* take part, as positive and efficient agents, in like manner as they do, in the modification of various sorts of dead matter? In the formation, for example, of a leaf, a blossom, or any sort of fruit; and in the formation of any animal fluid, tissue, or organ, of the blood, the bile, the urine, a muscle, the eye, the kidney, the liver, the brain, or any other kind of organized and living animal or vegetable matter—is it contended, that, in the formation of these products, the chemical forces act as they do in the formation of granite, marble, siliceous crystal, sulphuric acid, sulphate of iron, or any other sort of dead matter belonging exclusively to the mineral kingdom?"

Such are the interrogatories, which, to ascertain, if practicable, the meaning attached to the phrase "vital organic chemistry," by the chemico-physiological sect in medicine, I proposed through the press, eight years ago; and though the production containing them has been read by multitudes, they remain unanswered. Nor do I think it probable that they will ever be reduced to a different condition, by any of the members of the chemico-physiological sect. Nor are those gentlemen without a reason I suspect for remaining silent. An answer, whatever might be its spirit or matter, might be shown to be prejudicial to either their hypothesis, their own judgment or consistency, or to the three combined.

But an explanation, real or reputed, of the phrase in question, I must receive from some source, else it will be impracticable for me to proceed with my report; because I cannot rationally report or comment in any way on that to which I am a stranger. I shall therefore endeavor to derive some portion at least of the intelligence I need from the writings of Professor Liebig, who bears to the chemico-physiological sect the high and authoritative relation of Grand Master. His *dictum* on the subject, I shall on that

account regard as authentic. From his celebrated work entitled "Animal Chemistry," or "Organic Chemistry," I extract the following passages, and could extract dozens more tending to precisely the same purport.

"In the animal body we recognize as the *ultimate cause of all force*, only *one cause*, the CHEMICAL ACTION, which the elements of the food, and the oxygen of the air exercise on each other. The only known ultimate cause of *vital force*, either in animals or in plants, is a CHEMICAL PROCESS. IF THIS BE PREVENTED, THE PHENOMENA OF LIFE DO NOT MANIFEST THEMSLLVES. * * * * All vital activity arises from the mutual action of the oxygen of the atmosphere and the elements of the food."

"Physiology has sufficiently decisive grounds for the opinion, that every motion, every manifestation of force, is the RESULT OF A TRANSFORMATION *of the structure or of the substance of the animal body.*"

[But that motion or transformation, I reply, is effected, not by a chemical force; but by a superior force, which *controls* that of chemistry, and commands, modifies, and adapts to its own nature the transformation and its effects.—*Au.*]

"In the processes of nutrition and reproduction, we perceive the passage of matter from a state of motion to that of rest, (static equilibrium.) Under the influence of the nervous system, this matter enters again into a state of motion. The *ultimate* causes of these different conditions of the *vital force* are *chemical forces.*

"The power to effect transformations does not belong to the *vital principle.* Each transformation is owing to a disturbance in the attraction of the elements of a compound, and is consequently a *purely chemical process.*

"The combinations of the chemist relate to the change of matter, *forward and backward to the conversion of food into the various tissues and secretions, and to the irmetamorphosis in lifeless compounds.*"

[The only common sense construction of this passage is, that the same forces or powers of action, which at one time produce a *living*, at another produce a *dead* form of matter. In other words, that there is no difference between a *living* and a *dead result*, and therefore, that *difference* and *identity*, *opposition* and *concurrence* are the same.—*Au.*]

"How beautifully and admirably simple, with the aid of these" (*chemical*) "discoveries appears the process of *nutrition* in animals, the formation of their organs, &c.

"The self-regulating steam-engines furnish no unapt image of what occurs in the animal body.

"The animal body, in regard to the production of *heat* and *force*, acts like one of these engines.

"The carbon of the blood, which is converted into carbonic acid within the body, must give out exactly as much heat as if it had been burned *in the air*, or in oxygen gas.

"If chemical action be excluded, as a condition of *nervous agency*, it means nothing else than to derive the presence of *motion, the manifestation of force, from nothing.*

"The most decisive experiments of physiologists have shown, that the process of *chymification is independent of the vital force*; that it takes place in virtue of a purely chemical action, exactly similar to those processes of *decomposition and transformation*, which are known as PUTREFACTION, FERMENTATION, OR DECAY."

[Had I time to comment on this passage, I would undertake to establish decisively three points, both interesting and important:

First. That chymification is not a *chemical*, but a *vital* process.

Second. That no vital function or form of action bears the slightest resemblance to putrefaction or decay. And

Third. That, instead of benefiting genuine physiology by his experiments on St. Martin, Dr. Beaumont has seriously injured it.—*Au.*]

"The power of elements to unite together, and to form peculiar compounds, which are generated in animals and vegetables, is CHEMICAL AFFINITY.

"We should not permit ourselves to be withheld, by the idea of a VITAL PRINCIPLE, from considering, in a CHEMICAL POINT OF VIEW, the process of TRANSFORMATION of the food, and its ASSIMILATION by the various organs.

"We know that an organized body cannot *generate* substances, but only change the mode of their combinations, and that its SUSTENANCE and REPRODUCTION depend upon the CHEMICAL transformation of the matters which are employed in its nutriment, and which contain its own constituent elements. Whatever we regard as the causes of these transformations, the act of transformation is a PURELY CHEMICAL PROCESS."

Such are the extracts I have derived from Professor Liebig's Animal Chemistry. And, as heretofore mentioned, I could derive from it, I know not how many more of *the same import.* And its author is of course, as already stated, the oracle of his sect, whose responses and dictates few of his followers venture to contest.

Now, although these extracts do not of themselves amount to a real definition or even interpretation of the expression, Vital Organic Chemistry; they include the elements, out of which one may be constructed. They show conclusively, that Professor Liebig, however he may occasionally express himself, on the subject of a vital principle, considers and virtually declares every process, whether general or special, that is performed in the living system of man and other animals, and in vegetables also, to be performed exclusively by *chemical agency.* They make it clearly appear, that he regards both the *composition and decomposition,* the *building up* and *pulling down* of living organic matter, as a *purely chemical process.*

By chemistry alone, according to his teaching, is the food of man and other animals converted into chyme, the chyme into chyle, the chyle into blood, and the blood

into nerve muscle, bone, and every other tissue, as well as into bile, saliva, urine, cutaneous perspiration, and every other secretion and excretion of the system. In truth, from the showing and acknowledgement of the Professor himself, not only, according to his creed of human construction, is man originally formed by chemical agency; he is subsequently, in his whole economy, as exclusively governed and kept in play by it, as is a "*self-regulating steam engine.*"

Nor, under the light of a rigid and correct analysis of the subject, is this representation less applicable to his *mental* than to his *corporeal functions.* Chemistry is virtually made to give birth, modification, and sustenance to the whole of them.

For if there be in existence a more unscrupulous system of materialism than that contained in Liebig's Animal Chemistry, I know not where it is to be found.

While one sacred writer declares man to be, in his compound capacity, "fearfully and wonderfully made;" and another that he is 'made but a little lower than the angels,' the Professor of Giessen, in his account of him, makes him appear but little higher in his nature and being, than a German stove, or a Belgian beer-barrel! Had not experience and observation therefore abundantly taught me, that true christianity and religious partyism of a ferocious cast are necessarily in a state of belligerency toward each other, it would surprise me to find in the train of his followers and defenders scores of sulphur-lipped fanatics and bigots.

Nor is Liebig the only celebrated chemico-physiologist that represents man as a chemical product. Muller, Roget, Prichard, Carpenter and others of the same school, have maintained the same notion.

Muller, in a special manner, asserts that the seminal or generative substances which the male and the female contribute toward the immediate production of the embryo

(the *semen masculinum* to be planted and the *feminine ovariaas the soil* to receive and nourish it) to be "*produced from the proximate compounds of the blood, by a chemical process.*"

On this ground the embryotic ingredients contributed, compounded, and constructed, by the generative process, are also of necessity chemical. And so must be the issue they jointly engender. Nor, in compliance with an irreversible law of nature, can the *chemical embryo*, thus begotten and sustained, fail to grow up into *a chemical man.* So utterly ludicrous is the web of false doctrine, in which every thorough-going chemico-physiologist necessarily entangles himself! And this is as applicable to Roget, Carpenter, and Prichard, as it is to Muller.

I am now prepared to enter on the business of my report; because I now know respecting the subject of it all that I am at present destined to know.

From the contents of the writings of Liebig, Muller, and the other chemico-physiologists referred to, concurring with sentiments I have myself long entertained, and often expressed, I shall define Vital Organic Chemistry to be the groundless notion that—

Genuine chemistry can and does, without the cooperation and aid of any other force or principle of action, generate and sustain living organic matter, in the forms and capacities of vegetables and animals.

True, when the advocates of this notion speak of it in general and theoretic terms, they connect with it a superior controlling and plastic force, which they denominate *vital.* But when they describe specially and practically any function of living organized beings, they refer its production to a force *purely chemical.* And, singular as it may appear, it is notwithstanding true, that several, if not the whole of them, when pressed on the subject, pronounce the superior and plastic additional force to be the immediate influence of the WILL OF THE CREATOR---an act

or rather an artifice amounting in them to an abandonment of reason and science, and the adoption, as a substitute, of an expedient of rank superstition and fanaticism. It is virtually proclaiming every function of animals and vegetables to be something *above the operation of natural causes*, and *therefore miraculous*—all which shall presently more fully appear. Meantime, from the considerations on the subject hitherto adduced, I am inclined to report, that,

In my judgment, Vital organic chemistry cannot be justly called a branch of science. It is a MEANINGLESS NOTION.

And this report I shall endeavor further to substantiate by conclusive arguments. To execute this measure the more effectually, my first act shall be, to attempt the refutation of the most distinguished chemico-physiologists, who, in their writings on the subject under consideration, have offered sentiments contradictory of mine. And the most effectual mode of overthrowing an opponent being to set him irreconcilably at war with himself; my design is to refute my antagonists, by arguments derived from their own productions on the topic I am considering. In other more express and forcible terms, I shall, by making it clearly and conclusively appear, that their own arguments (if such they may be called) are self-destructive, I shall affix on them the felony of *literary suicide*. And, as the most prominent and anthoritative of the corps, I shall begin with Professor Liebig. In his work on Animal Chemistry, I find the following passages, which will be perceived to be directly opposed in sentiment to those which have been just quoted from the same volume.

"A rational physiology cannot be founded on mere reaction; and the living body cannot be viewed as a *chemical laboratory*.

"With all its discoveries, modern chemistry has performed but slendor services to physiology and pathology.

"In the animal ovum, as well as in the seed of a plant, we recognize a certain force, the source of growth, or in-

crease in the mass, and of reproduction or of supply of matter consumed. * * * This force is called the *vital force*, the *vis vitæ*, or *vitality*.

"He is not the true chemist, who endeavors to apply to the animal organism his notions derived from purely chemical *processes*. He has not the remotest intention of undertaking the explanation of any really *vital phenomenon* on *chemical principles*.

"In what form, or in what manner the vital force produces mechanical effects in the animal body, is altogether unknown, and is as little to be ascertained by experiment as the connexion of chemical action with the phenomena of motion.

"So it is with vital force, and with the phenomena exhibited by living bodies. The *cause* of these phenomena is not *chemical force*; it is not electricity or magnetism. It is a *peculiar force*, because it exhibits manifestations which are formed by *no other known force*.

"The vital force appears as a moving force or cause of motion, when it *overcomes* the chemical forces, cohesion, and affinity, which act between the constituents of food * * * * The vital force is manifested as a cause of motion in *overcoming* the chemical attraction of the constituents of food, and is also the cause which *compels them to combine in a new arrangement*."

Though I could adduce, from Animal Chemistry, scores of other passages to the same purport with the foregoing, the labor would be superfluous. Those already quoted more than neutralize—they utterly *demolish* the work, as a chemico-physiological production. To prove that a publication is *self-inconsistent, is to destroy its reputation and influence*—the penalty thus irrevocably inflicted on Professor Liebig's book.

I shall now expose in the same way, to the same extent, and on the same subject, the errors of Dr. Roget and Dr. Carpenter.

"However," says the former of these distinguished

writers, the laws which regulate vital phenomena may appear, on a superficial view, to differ from those by which the physical changes taking place in inorganic matter are governed, still there is really no essential difference between them." * * "It may, in like manner, be contended, that the affinities which hold together the elements of living bodies, and which govern the elaboration of organic products, *are the same* with those which preside over inorganic products."

So much for that gentleman's views, as a chemico-physiologist, in relation to the production and maintenance of living organic matter. Let us now contemplate his views on the same subject, as a vitalist, compare them with those just stated, and judge of the contrast.

"The foundations," he says, "of the edifice" (of the chick in the egg) "are laid in the homogeneous jelly, *by the efforts of the vital powers.*" "At first," he continues, "all the energies of *vitality* are directed to the raising of the fabric, and to the extension of those organs, which are of greatest immediate utility; but still having a prospective view to farther and more important ends," and so on to the close of the process, the whole work of developing, fashioning, and locating the fœtal organs being assigned exclusively, to the "*efforts of the vital powers,*" and to the "*energies of vitality;*" *chemistry having, from beginning to end, no agency in the work.* In another part of the same publication, Dr. Roget correctly asserts, that the products of the *vital powers*, in their *formative processes*, are uniformly *organs, designed and fitted for the performance of given purposes;* while the products of chemical forces are uniformly both *organless* and *designless.*

And, for a further distinction equally essential, between vital and chemical products, he might have correctly added, that the former are uniformly bounded by curve or waving lines; and the latter by straight lines and angles.

Dr. Carpenter, whose ability and celebrity as a writer, neither need commendation from the pen of a friend, nor dread censure from that of a foe; when under the influence of his chemico-physiological mood, writes as follows:

"Reason has been already given for the belief, that the affinities which hold together the elementary particles of organized structures are not different from those concerned in the inorganic world; and it has been shown that the tendency to *decomposition*" *(putrefaction)* "after death, bears a very close relation with the activity of the *changes that take place in the part during life.*"

But, when in the mood of a vitalist, which occasionally visits him, the same gentleman writes to the following purport:

"Organization, and vital properties are simultaneously communicated to the germ by the structures of its parents. Those vital properties confer upon it the means of itself assimilating, and thereby organizing and endowing with vitality, the materials supplied by the inorganic world."

And in the same spirit he further says:

"The agency of vitality does not change the properties of the elements" (of the food and the blood) "but simply combines them *in the modes which we cannot imitate.*"

The late Dr. Prichard, one of the most keen, close, and elaborate thinkers and writers on matters of medicine that England has produced, was several years ago a bold and thorough-going chemico-vitalist. But a further examination of the subject now under our consideration, induced him so far to alter and modify his views, that, in one of his later publications, he thus expressed himself:

"We may, if we choose to do so, term the cause which governs the organization and vital existence a *plastic prin-*

ciple; but it is a principle *endowed with intelligence* and *design.* It is in fact nothing more than the ENERGIES OF THE DEITY. The development of generic, specific, and individual forms, as well in the vegetable, as the animal kingdom, can be accounted for only, by ascribing it to the universal energy and wisdom of the Creator."

Yet was the Doctor, I say, at one time, a firm believer in the doctrine (or rather *notion*) of *chemical vitality.* But having, from his own superior penetration and sagacity, become thoroughly convinced of the fallacy and preposterousness of that hypothesis, and perceiving the absolute necessity that a plastic power and not a mere power of *chemical affinity* must be employed in the construction of living organized matter—under these impressions, actuated by some unaccountable crotchet, which, at times, takes possession of the most distinguished minds, instead of looking for the requisite plasticity in a *secondary* cause; he referred it at once to the GREAT CAUSE of causes; thus representing the development of even a blade of grass or a leaf of mucor as a *miraculous event!*

Having thus shown that the leading chemico-physiological writers referred to have, by self-contradiction, refuted themselves, in relation to the subject under consideration, I shall endeavor to strengthen that refutation, by a few arguments drawn from my own resources. And the matter of those resources will be derived from the conditions and manifestations of the human body in a living state.

But prevented, by a want of time, from including in my discussion of the subject, all the functions of the system of man, and making it appear that a belief in the existence of vital organic chemistry, as a branch of science, is forbidden by the nature and economy of the whole of them; I shall confine my remarks principally to one of the most prominent and important of them, and the attribution of whose *origin* and *continuance* to the influence of chemical

forces involves less absurdity than a similar notion respecting the attributes of any other sort of vital action.

There are especially two of the functions belonging to the system of man and other warm blooded animals, which constitute at present the chief topics of contention between the vital and the chemico-physiological sects of the medical profession. They are *calorification* and *chymification*. And I am in my own unqualified opinion, prepared to show, beyond denial or doubt, that neither of them is explicable on chemical principles. But as I cannot, in my consideration of the question before me, embrace them both, I shall select the former. And Liebig being, as already stated, the leader of the sect opposed to me, I shall confine my remarks principally to his dogmas on the function to be considered. Should I satisfactorily establish the insufficiency of them, my work will be done. He being vanquished, his followers may be expected to surrender at discretion.

The fundamental assumption of the Professor is, that the heat of the human body and of the bodies of other similarly organized animals, like the heat of a German stove, is the result of combustion. And in this he is correct; except that, in the former, carbon and hydrogen are both burned; but in the latter carbon perhaps alone. Another of his dogmas is, that the same amount of carbon, whether burned *in* the human body, or *out* of it, always evolves the same amount of caloric, and produces of course the same degree of heat. But in this opinion I shall endeavor to show hereafter, that he is incorrect. I shall unquestionably show, that, if his opinion be not, in all respects, essentially unfounded, it certainly involves at least one very striking phenomenon utterly inconsistent with the principles he advocates.

In his fundamental propositions and his comments on them toward the establishment of his doctrine of animal heat, he has laid down one in particular, and contended

for its validity, which I must specially notice; because it is, in my judgment, as groundless as it is important—being, in both respects, of a high order.

That their bodies may retain the requisite temperature, it is necessary for the inhabitants of the frigid zones, especially during the winter season, to generate a much greater amount of heat, than it is for those of the temperate and the torrid zones. To enable them to do this, Liebig contends, that in the former region, persons possessing chests and lungs of the same capacity, inhale, in a given time (because the atmosphere around them is colder) a much greater quantity of oxygen, than do those of the latter region, the atmosphere around them being much warmer.

This hypothesis, though plausible, and very extensively, if not universally believed, I pronounce utterly erroneous. And my reasons for thus characterizing it, are so clear and substantial, that I deem it impossible for any one of even common ability of mind to fail, on due consideration of them, to acknowledge their perspicuity as well as their truth. In testimony of this I shall briefly detail them.

In each climate, torrid, temperate, and polar (and this Professor Liebig himself admits) the same quantity of air makes its way into the lungs, in the same space of time. And he farther correctly admits, that by the time the air has reached the interior of the lungs, its temperature is the same (99 or 100° of Fahrenheit) whatever may be the temperature of the surrounding atmosphere from which it is derived. And I myself, (on the ground of experiment) positively add, that however cold may be the atmosphere from which it is extracted, the air inspired has attained the acme of its temperature (99° or 100°) even before it has passed the *rima glottidis.*

No matter then from what atmosphere it is drawn (polar, temperate, or tropical) if the air inspired (being the

same in volume) possesses, when it enters the bronchiæ and their air-cells, the same temperature, it contains also the same amount of oxygen; which again can unite with and burn up only the same quantity of carbon and hydrogen. Nor are these all the identities that characterize it which the occasion urges me to notice.

With entire truth and confidence may I add, that (in direct contradiction and overthrow of the assertion of Liebig) whatever may be the temperature of the incumbent atmosphere, the air drawn from it, by inspiration, possesses when in the lungs the same degree of humidity.

This assertion I make to encounter and defeat the design and aim of the allegation by Liebig, that, in the warm atmosphere of the tropics, moisture occupies and fills up the place of oxygen in the cold and dry atmosphere of the polar region.

Admit the truth of this position as respects the air *without* and *around* the persons of the inhabitants, it is not true of the air in their lungs. As already stated, the air in the bronchiæ and air-cells is always of the same temperature, whether the weather be warm or cold; and the secretion from the internal surface of the lungs maintains it in the same degree of humidity, whether the weather be moist or dry.

Did the Professor of Giessen moreover understand meteorology as well as he ought to understand it, before undertaking to excogitate a new doctrine in it, he would not need to be told that the driest condition the atmosphere ever attains exists when the harmattan wind blows. And that prevails in its high degree only in the torrid zone, and chiefly, if not solely during the hottest season of the year.

It is not true therefore, that the tropical atmosphere contains less oxygen than the polar, because it superabounds in humidity. It contains less *when* and *because* it is more expanded by its more elevated temperature. For

the amount of oxygen in a cubic foot, yard, or other measure of the atmosphere is always and necessarily (other things being alike) in precise accordance with such expansion. And in the lungs of a living and healthy human being unadulterated atmospherical air is uniformly in the same condition.

The truth of this position, if it be held in doubt, I freely stake on the following experiment.

Let the ablest analytical chemist of the day, (Professor Liebig himself,) during ordinary weather, take from each of two bodies of the atmosphere, one of them at the temperature of zero, and the other at that of 80° of Fahrenheit, a cubit foot of air. Let him heat these two extracted portions to the temperature of 100°. From each of them again let him take an equal quantity by weight or measurement, and accurately analyze it, and he will find in each the same amount of oxygen, and of water in the form of vapor. For, come from what climates they may, equal quantities of atmospheric air, of the same temperature with each other, possess equal quantities of oxygen, as well as of nitrogen and carbonic acid gas, and also of humidity under similar circumstances—of humidity I mean in the air-cells of the lungs.

In asserting therefore that individuals possessed of chests and lungs of the same size, and breathing an equal number of times in a minute, inspire, in a given space more, oxygen in cold climates than in warm—in Archangel than in Calcutta,—Professor Liebig has committed a mistake, which he ought publicly to renounce, and no longer mislead his credulous followers. Yet does the *vital* process of respiration *(because it is vital)* evolve, from an equal quantity of calorific materials, a greater amount of caloric in the winter atmosphere of Labrador, than it does in the winter atmosphere of Panama; or in that of any part of tropical Africa. And this I say it does under the controling influence of a principle or force of a higher

order and character (I might say of a higher order of *being*) which enables it to produce the effect—that effect being indispensable to the economy of living matter. Nor is this the only error, respecting animal heat, of a nature somewhat similar, into which that Professor has fallen.

Because infants possess lungs more voluminous, in proportion to the size of their systems, than adults do, the Professor has asserted that they also evolve by respiration (in the same proportion) more caloric, and therefore possess a higher temperature.

Let the validity of this notion be tested by the following experiment, and I venture to pledge my experience and observation that it will be found wanting.

Expose to a cold and humid atmosphere, under a covering equally light and scanty, an infant and an adult alike healthy and robust for their age, (each remaining equally quiet) and mark the issue. Before the adult will be, in any perceptible degree affected, the temperature and pulse of the infant will begin to decline. On this point unerring instinct correctly informs the inferior animals. Hence the attention and care with which the mother protects her young from the cold. And for want of attention similarly affectionate and wise, thousands of infants suffer and perish. This clearly shows that the quantity of heat evolved is not, in all cases, proportionate to the qnantity of oxygen employed in respiration. And to me the following facts appear to testify to the same purport.

The chest and lungs of some adults are much more capacious, in proportion to the size of their whole systems, than those of others; and they inspire as fully and frequently. They inhale, of course, in a given time, a greater amount of oxygen; and neither their perspiration nor any other discharge by them is more copious. Yet is their temperature no higher.

On the ground of his own experiments, we are assured

by Treviranus, a writer of reputation and credibility, that, in an atmosphere at the temperature of 80½° Fahrenheit, the honey bee exhales three times as much carbonic acid gas, as it does in an atmosphere at the temperature of 50½°. Yet, may we confide in the doctrine of Professor Liebig, and other chemists, the insect inspires, in the latter case, the greatest amount of oxygen; and we do not know, nor have we any reason to believe, that it consumes, in its aliment, less carbon. Nor when the atmosphere is at the temperature of 50½° is the honey bee at all impaired, or in any way wanting in the functions of vitality.

Muller informs us, on the authority of experiments he has performed or witnessed, that some cold-blooded animals inhale *three times* as much oxygen as is requisite to form the amount of carbonic acid gas which they discharge. Will Professor Liebig, or any of his followers inform us, how the superabundant quantity is disposed of by those animals?

Muller again informs us that man exhales a greater amount of carbonic acid gas when the barometer is *low* than he does when it is *high*. Yet does the air contain, in a given volume, a larger portion of oxygen in the latter case, than it does in the *former*, and of each of its other ingredients also a higher ratio.

The human female expires a larger quantity of carbonic acid gas when in a state of pregnancy, than when in an unimpregnated one. Yet does she inhale less oxygen, especially during the latter stages of her pregnancy, on account of the diminished expansion of the lungs, owing to the augmented pressure on them by the enlargement of the uterus.

These facts are derived from the economy of animated nature, in a healthy condition. Let us attend to the phenomena exhibited by it, in the same respect, in a state of disease.

The temperature of a patient, in an advanced stage of

typhus fever, when he is greatly debilitated, is often as high as 105° or 106° of Fahrenheit, and, at times higher. Yet is his respiration exceedingly restricted on account of debility. He inhales, in a given time, less oxygen by a third or fourth part, than he does in health; and from its emaciated condition, his body is equally deprived of carbon.

In cases of asthma and hydrothorax the same is true. Respiration is preternaturally confined, and the temperature at times, though not perhaps always, preternaturally high.

The following passages, on the same subject, are extracts from my "Physiology Vindicated;" a work published in the year 1843; and no refutation or exposure of their errors, possessing the least merit of any description that has ever been attempted.

"But it is more especially by the phenomena of phthisis pulmonalis that the hypothesis of our author" (that now under consideration) "is irretrievably overthrown. When sufferers under that complaint are able to breathe with but a small portion of a *single lung*, when they have not in their entire systems an ounce of fat to furnish carbon, and but comparatively little water to furnish hydrogen; and when the amount of their food and drink (especially the former) is exceedingly small, and none of it oily—under this destitution of all materials essential to combustion—as well of oxygen to consume as of "fuel" to be consumed—they manifest, for weeks and even months, a high degree of febrile temperature, amounting to 105° or 106° of Fahrenheit—while it should, according to the hypothesis we are examining, be far below the human temperature of health. Were there no other argument but this, to urge against our author's notion, that the vital temperature is the result of *common combustion*, it would alone completely demolish it."

Scores of cases of phthisis pulmonalis, of this descrip-

tion, have fallen under my own observation. In corroboration of their truth, the following very extraordinary case of the same complaint (not seen however by myself) is referred to, in a pamphlet, which I published in the year 1844. I here extract a brief account of it, from that production.

The case is recorded on the authority of Graves and Stokes of Dublin, and appears, in the 5th volume of the Dublin Hospital Reports. It is also referred to, in the American Journal of Medicine, vol. 8, year 1831, p. 218, and is as follows:

"Each lobe of the lungs was extensively tuberculated; the pulse beat 126 times in a minute; and the temperature was *highly febrile*. Yet did the patient respire but *fourteen times* in a minute!"

To contend that the temperature of this patient, under this case of disease, was produced by the *common combustion* of carbon and hydrogen, I pronounce preposterous! As well may a disputant exercise his sophistry, in contending for an effect without a cause, or for the existence of two adjacent hills without a hollow!

I well know that, by some of my opponents, it is contended that, in these cases of inordinately *augmented* febrile heat and *diminished* respiration, the excess of temperature is attributable to the *want* of perspiration. But I as well know that the *cause alleged* is far too *limited and feeble* to account for the magnitude of the effect produced. And a fact strongly analogous to this I shall demonstrate by *count and admeasurement*, which, when accurately executed, never deceive.

Nor is the following representation less irreconcilable with the dogma of Professor Liebig. I quote again from a publication of my own, which has also lain uncontradicted, since the year 1844.

"Sir Benjamin Brodie ascertained by experiments, whose accuracy is not doubted, that a rabbit full grown and healthy, expires every half hour, 28 22 cubic inches of carbonic acid gas. And when the same rabbit is destroyed by poison, or by bisecting the medulla oblongata, and artificial respiration is kept up in it, a quantity of the same gas, varying from 20.24 to 25.55 and 28.27 cubic inches continues to be exhaled in the same length of time. Yet does the temperature of the animal in the course of an hour, fall from *six to eight degrees.*

"Some experimenters assert that, under the influence of artificial respiration, the temperature of the dead animal falls more rapidly, than when the process is not employed.

"At any rate, the experiments of Sir Benjamin Brodie, if accurately performed, and correctly reported, satisfactorily show, that when the *vitality* of an animal is extinguished, its temperature falls, notwithstanding the production *in* and the elimination *from* its system, of the usual quantity of carbonic acid, by artificial respiration. And they also show conclusively, that the vital forces, whatever may be their nature or essence, are capable of maintaining animal heat by some agency other than that of *chemical combustion.* And that therefore the economy of a living human body is not identical with that of a German stove, the authority of Liebig to the contrary notwithstanding.

"Facts like the foregoing I could continue to detail almost interminably. And each of them would be alike fatal to the wild, dashing, ill-concerted and worse-supported hypothesis of Liebig and his followers."

But I shall content myself for the present with a statement of the following experiments and the observations accompanying them, which are exclusively my own. As far as my information and belief on the subject extend, the first conception and performance of them were mine. Should their disclosure and record therefore eventuate in usefulness and merit, the issue will pass to my credit. And in case the reverse be their lot, I shall without a murmur submit to the blame.

I here exhibit to the reader the figures of two extraordinary men; Calvin Edson, the "WALKING SKELETON," and J. T. Reed, the "CANADIAN GIANT," whose persons I have seen, and whose characters, habits, and conditions I have personally examined, and carefully studied. Edson's weight was *fifty-seven pounds*, and Reed's but little less than *six hundred;* and the difference between their ages was but inconsiderable in extent--certainly far from being sufficient to mar in any way or degree, the fairness of the experiment and comparison made when I saw them, and now about to be accurately described.

Notwithstanding the prodigious difference in the size of their persons, that in the natural capacity of their chests and lungs was very moderate. In consequence therefore, of the impediment necessarily presented to the play of Reed's lungs by his immense obesity, it can hardly be doubted that his respiration was inferior in compass to the respiration of Edson. Even a presumption that it *might* have been *superior* is *inadmissible.* Let it be conceded however, that the quantities of oxygen they inhaled, in a given time, were equal—as they inspired with about equal frequency.

In neither the kind nor amount of their diet and drink was there any material difference. And of their secretions and excretions the same is true. Singular and perhaps incredible as, to some persons, the fact may appear,

there was no authentic evidence, that Edson discharged from his system, through any of its outlets, a greater

portion of the inherent ingredients of it, than Reed did from the same outlets of his. In a special manner, there was no evidence that he discharged more by *perspiration.*

It may be said therefore, without any material departure from fact, that the two individuals, of whom I am speaking, conveyed into their systems, through the trachea and the œsophagus, the same kind and quantity of matter, solid, fluid, and aeriform. To imitate the language of Liebig—the same amount of *fuel* to be consumed,

and the same amount of oxygen to consume it, for the purpose of heating their "*body stoves*," was regularly appropriated to that end. Nor was there perceptibly a greater escape of their product from one of them than from the other.

But the difference in the amount of matter, of which those stoves consisted was immense—and fatty or oily matter deposited in the apparatus of Reed excepted, the kinds were the same. Nor *did I then, nor do I now* venture even to conjecture, from what source such a huge mass of oleugenous matter was derived. But *there* it was; and *there* was also the skin and bone fabric of Edson extracted from a similar repository.

It was necessary for Edson to raise his stove, which weighed but little more than fifty pounds to the temperature of 100° Fahrenheit; and he did so; and there the process was checked. And it was equally necessary for Reid to heat his stove, which weighed near six hundred pounds, to the same temperature, else ruin must ensue. And he performed the deed by the same extent of combustion, as his means of action. But, to drop metaphor, and resume directness of speech. With a given amount of oxygen, carbon, and hydrogen Reed raised to 100° of Fahrenheit, the temperature of near *six hundred* pounds of *living human substance;* while, with the same amount, Edson raised to the same height, *but fifty-seven pounds.* Working therefore with the same means, Reed surpassed Edson, in operation and effect, *more than tenfold.*

Such I say was the case, while the bodies of the two individuals were living. But suppose them to be lifeless and cold. In that condition, diffuse through the carcass of Reed the quantity of caloric generated by Edson while living, and through the carcass of Edson the quantity evolved during life, by the system of Reed. What will be the effect? The answer is equally plain and easy. By the operation, the dead body of Reed will not be raised in

temperature *ten degrees;* while the chief portion of that of Edson will be *reduced to cinders*—all in it that is exhalable being turned into gas. Suppose again the two men to be living, and the following experiment to be performed on them.

Let the body of Reed be suddenly deprived of all its caloric, and the caloric of Edson be transferred to it; while to the body of Edson, deprived of *its* caloric, the caloric of Reed is transferred. The result of this exchange is obvious. Edson will be instantly *destroyed and fricasseed by heat*; and Reed *converted by cold into a frozen mass.*

Of these several experiments here submitted, my business is to guaranty the truth; which I deliberately do. Let candid and intelligent men conscientiously apply it, under the guidance and sanction of their knowledge and judgment, to the subject of my report, and I shall cheerfully and confidently await the issue.

The facts I have here clearly, confidently, and veritably stated, make it incontrovertibly appear, that, contrary to the notion commonly entertained respecting it, vital calorification is as little understood, and perhaps more generally and flagrantly misrepresented than any other function of living organic matter.

When therefore a chemico-physiologist presents himself to an audience as a speaker, or to the public as a writer, and gravely assures them, as Liebig, his followers and associates have long done, and as hundreds of teachers are still doing, that vital temperature is the result of *common chemical combustion*—whoever thus acts, becomes by the deed an appropriate object for

"Scorn to point her slow unmoving finger at."

There are at this time, in Louisville, two females, one of them white and the other colored, weighing each three hundred and fifty pounds. They are also each about fifty-five years old; and their temperature is that of na-

ture and health. I have carefully examined them both, and ascertained from them the following facts:

Until the age of near forty years, neither of them weighed more than one hundred and twenty-five pounds; and their diet and drink then were, in quantity and kind, the same that they are at present. And that their temperature was also the same is not to be doubted; because, as just mentioned, it is now *natural.* And it is presumable that it has been always the same; because they have been always healthy. Nor do they certainly inhale now more oxygen than they did when thirty years younger—probably not so much—their respiration being somewhat impeded and contracted by obesity. Their bulk moreover is nearly three times as large as it was in their youth. Hence, with an equal stock of calorific materials, they raise *now*, at the age of fifty-five, to the same temperature, nearly three times as much animal substance as they did at the age of twenty-five. This neither is, nor can be a deed performed by common combustion, the result of the operation of mere chemical affinity. Nor can I suppress my astonishment, that, at the middle of the nineteenth century, a period so signalized by the knowledge of nature, a contrary belief is seriously entertained by any enlightened and reflecting individual.

My astonishment on this subject would be less than it is, were facts precisely similar to those I have related, less numerous and common than I know them to be. For I doubt whether there can be found, in the United States, a single town or city containing ten or even five thousand inhabitants, where instances of a like description and import do not exist. But to conclude my report.

As a legitimate inference from the facts, illustrations, and arguments contained in this paper, I feel authorized to repeat what I have heretofore said in substance; that "VITAL ORGANIC CHEMISTRY" is a form of expression denoting nothing that actually exists. I am therefore,

compelled to regard it in my best judgment, as mere *verba inana, et preterea nihil*—an empty association of words, which error and a forbidden assumption of knowledge have engendered; and which truth and genuine science disown. All which is respectfully submitted.

P. S.—In my own system a phenomenon has recently occurred, and still exists, which to my mind demonstrates no less conclusively that vital temperature is modified by an influence different from and superior to anything that belongs to the heat of common combustion.

In the great sciatic nerve I suffered recently an uncommonly violent attack of neuralgia, entirely exempt from any augmentation of vascular action, from the effects of which I have not yet recovered. In a special manner the affected limb is not a little weakened in its motive power. And, to say the least, the force and circulation of the blood in it, if not diminished, are certainly not augmented.

Under these circumstances, it was natural to expect, and I did expect that the temperature of the diseased and now debilitated limb would fall below that of the sound and more vigorous one.

But not a little to my surprise, and no less to my gratification and rejoicing, the reverse is now true, and has been so since the commencement of the disease. The sound limb is coolest by at least one degree, if not by one and a half. I say that this phenomenon has occurred *to my gratification and rejoicing.*" The cause of my being thus affected is sufficiently plain. The superior warmth of the diseased limb bespeaks in it a degree of vitality, which promises to minister towards restoring it to health and strength.

Whatever may be the case with regard to others, in contemplating this event, I see or fancy I see in it a striking discrepance between the production of vital temperature, and the temperature of ordinary combustion.

That I may be neither misunderstood nor misrepresented on this subject, I must, if possible be more explicit.

I do not deny that chemical forces constantly and necessarily operate and do much in the living body of man and other animals. On the contrary, I know and acknowledge that they do. But they do not operate *plastically* with respect to *living organized matter*. They neither *organize* such matter, give it figure and organic fitness, nor endow it with life. That work is performed by a higher and controlling power, denominated *vital*, which is emphatically *sui generis*.

Addendum.—The reader must have observed that, in my reply to Professor Liebig, I may have appeared to concur with him in the notion that the blood receives *all* its oxygen immediately from the lungs. But such is not the fact. I do not thus concur. Every article of our diet and drink contains oxygen to a greater or less amount. The blood is therefore supplied with that element through the nutritive no less certainly than through the respiratory function. And animal temperature is developed by vital action in every organ of the body, (the bones themselves not excepted) as indubitably as it is in the lungs.

Hence it was that, in my Inaugural Dissertation, dated in the year 1795 (fifty-seven years ago) I pronounced animal temperature to be "*a secretion*"—or rather a *condition of organic matter, the product of secretory action*, and its abstraction from the system by the application of cold water, cold air, or any other cold substance, to be as *genuine an evacuation* as perspiration, micturition, or the emission of blood by the puncture of a lancet. And antiquated and unfashionable as that doctrine may be now deemed, it was then, for a time, accounted orthodox in Philadelphia; and I still retain it as a sound tenet of my physiological creed. But it was not favored by Dr. Rush, because it clashed with some of his peculiar notions.

I therefore regard animal temperature as the product of a process *essentially and exclusively* VITAL. Nor do I hesitate to pronounce the chemical dogmas of the celebrated Professor of Giessen, when applied to living organic mat-

ter, whether vegetable or animal, as groundless and untenable as the wildest vagaries that ever issued from the tongue or the pen of that prince of boasters, fictionists and notion-mongers, the renowned AUREOLUS PHILLIPPUS THEOPHRASTUS BOMBASTUS DE HOENHEIM PARACELSUS.

I shall only add, that Dr. Woodhouse, a sound and strong minded thinker, and Professor of Chemistry, at the time of my graduation, in the Medical Department of the University of Pennsylvania, and one of the ablest chemists then in the United States, adopted my opinion respecting animal temperature. From the time of the publication therefore, of my Inaugural Dissertation in 1795, to the end of his life, he regularly taught the doctrine that the caloric generative of animal temperature is the product of a *bona fide secretory process*, and its passage out of the body a genuine evacuation.

To conclude, by a summary of certain sentiments I have heretofore expressed. I shall be neither surprised nor disappointed should some of the chemico-physiological sect of medical speculators cavil at and perhaps denounce my remarks on Professor Liebig's work on Animal or Organic Chemistry. I am prepared to find that such fault-finders and disparagers will be inclined to attack and condemn my strictures on that elaborate but (to me) obscure and equivocal production, because its author has not, in any part of it, clearly and fully recognized the existence of a *vital force or principle*, as an *indispensable attribute* of man, and other living organized beings, nor correctly recounted its uses and performances—because in fact he has not described it as the great *plastic power*, which, as the chief executor of the laws of heaven, for that special purpose framed and provided by heaven's supreme LAW GIVER—makes man what he is in structure, and enables him, through his endowment and aptitude, *to do what he does by his efficiency.*

Is any one of the sect referred to prepared to contend that Professor Liebig has sufficiently described and char-

acterized the Vital principle of man and other forms of organized matter, both vegetable and animal, in all the powers and fitnesses it possesses, and in all the achievements it performs in the diversified economy it exhibits in the wide field of organized matter?

I fearlessly reply that he has not. And I with equal fearlessness defy any physiologist of any sect, or all physiologists of all sects, to specify the page or pages of his book or books, in which the Professor has thus acquitted himself—in which he has fairly and fully represented the vital principle, as possessing the numerous high, varied and important attributes it does possess, and as performing the numerous high, diversified and important functions it does perform in the economy of life and organization.

That the Professor has at certain times, and for certain purposes, spoken of the vital principle *in general terms*, with propriety and correctness, is not now, nor ever has been denied by me. But when his remarks involve *particular points and specifications*, his spirit and purport instantly and utterly change; and what he had previously attributed to *Vitality*, he then transfers to *Chemistry*.

In consequence of this versatility of purpose and conduct, his Animal or Organic Chemistry is one of the most flagrantly self-inconsistent, and self-contradictory productions I have ever examined. Hence, in the progressive improvement of the science of anthropology, and other forms of living organized matter, the popularity and influence it once possessed and exercised are as certain to pass from it, as is the life which actuates and has sustained him in a sphere so ample, attractive and commanding, is destined to pass from the mortal fabric of its distinguished author.

The two first pages of my article on "Vital Organic Chemistry" were printed off without my having examined and corrected them. In consequence of this the reader will please to make for himself the following corrections.

In the third sentence from the top of page 104, for "humble" he will read, *simple*.

And in the fourth sentence for "apprehending and definitely comprehending," he will read,—*of penetrating* and definitely comprehending. C. C.

BIBLIOGRAPHICAL NOTICES.

ART. V.—*The Vital Statistics and Sanitary Condition of Memphis, Tennessee, an Anniversary Address, delivered by appointment, before the Memphis Medical Society, on the 5th of February,* 1852. By GEO. R. GRANT, M. D.

Dr. Grant, when he commenced the investigation, the results of which are embodied in this pamphlet, had the conviction, "that Memphis is one of the healthiest places on the Mississippi river." He believed that a comparison of the sanitary condition of that city with that of "other places in the great valley, and also with other cities in the Union, would conclusively establish the truth of this opinion." He says, he entered with alacrity on the self-imposed task of disproving by figures the charge, that Memphis is a sickly place, but on inquiry he was forced to conclude that, in this instance, common rumor had too much truth in it. We make a few extracts from his interesting address.

The *white* population of Memphis, according to the census of 1850, was 6,369. Of this number, 3,587 were males, and 2,782 were females. There were also, 44 free *colored* males, and 65 free *colored* females. As the deaths among these 109 free persons of color were not kept separate, but included in the returns among the whites as free persons, their numbers must be added to the whites, in order to arrive at the per centage of mortality among the persons, white and colored, composing our white population. Among this white and colored free population, the

returns give 238 deaths; a per centage of 3.67, or 36 in every thousand. From the same authority we learn, that the slave population numbered 2,362; of whom 116 had died; making the per centage of deaths among this class 4.91, or 49 out of every thousand. The average mortality of these two classes together, gives the astonishing result of a fraction over 4 per cent., or 1 death in every 25 living!

The returns of the City and County being kept separate, made it, comparatively, an easy task to examine the *vital statistics* of our immediate neighborhood, and therefrom collect materials wherewith to compare our sanitary condition with that of the people by whom we are surrounded. These statistics show a population in the county, exclusive of the city, of 10,317 free persons, and 11,998 slaves; making a total of 22,315. The deaths among the free were 190, being 1.84 per cent; and among the slaves, 279, giving 2.32 per cent. The average ratio of mortality among our neighbors turn, out to be only 2. 1-10 per cent of the entire population, or one in every 47 living. Here we have proved to us, by unmistakeable data, that whilst one in every 25 had died in town, the people who are separated from us by a mere compass line, have lost but *one*, by death, out of every 47 living. This difference in the mortality of the city over the county, of very nearly *two to one*, is well calculated to excite the surprise of the statist, and the sympathy of the philanthropist; while it offers to the medical philosopher an open and interesting field for etiological investigation.

If the foregoing statements are calculated to astonish us, what will we say when we compare our sanitary condition with that of New Orleans, as shown from the late census returns of that city, the vital statistics of which have been carefully collected by Dr. Barton, and published in the last volume of the "Southern Medical Reports." According to these returns, we find the mortality in New Orleans and Lafayette, *exclusive* of cholera, to be a fraction less than 2 per cent., (1.96) and *including* cholera, it is less than 2½ per cent; being 2.44. This shows a lower rate of mortality, by more than one and a palf per cent. in New Orleans than occurred in Memphis during the same period of time; the facts being collected in a similar manner; ours, by one of our own citizens, who is in every

respect well qualified for the proper performance of the duties the office enjoined.

It would be a fruitless labor to extend our inquiries to other cities of the Union, with the hope of finding in the census returns of 1850, any mortuary statistics at all approximating to the figures exhibited by the recorded deaths among our population, for the period embraced therein. Every comparison of the sort would only be to our disadvantage.

Dr. Grant finds that the mortality in the Memphis Hospital is also greater than in other similar institutions in our country.

The total number of admissions into the Memphis Charity Hospital for the year ending on the 31st of December last, was 474. The books show that 116 of these had died; making a mortality of 24.47 per cent; wanting a fraction only of being one out of every four, or very nearly 24½ out of every hundred! That this is a terrific mortality, will become quite evident by comparing it with the annual mortality witnessed in other Hospitals—some of them in localities confessedly insalubrious.

From estimates made from twelve years' observations by Dr. Playfair, in the Hospitals of Liverpool and Manchester, the mortality was 3.57 per cent., or one in every 28. From a statement published in the January number of the "London Lancet" for the present year, we learn that there had been admitted into all the Hospitals of Paris, during the year 1850, 84,044 patients. Of these, 6,855 had died; making an average mortality of 8.15 per cent., or one in every 12¼.

In the Marine Hospital at Louisville, Kentucky, as we are informed by Dr. Rogers, in the December number of the Western Medical and Surgical Journal of last year, the mortality was a small fraction over 7 per cent., or one in every 14.

According to a statement published by Dr. McKelvey, the Surgeon to the United States Marine Hospital at New Orleans, out of 1,116 admissions in the year 1849, there occurred but 64 deaths; making a mortality of only 5.73 per cent., or one in every 17. In the Charity Hospital,—

that immense lazaretto of New Orleans,—the admissions in 1850 numbered 18,676 patients, of whom 1,884 died—being a mortality of 10 per cent., or one in every 10.

From these comparative hospital statistics, it is but too evident that the mortality at our Hospital is more than twice as large as that of the Charity Hospital at New Orleans, which has always been considered, in this quarter, as the great Southern receptacle of the sick and the dying!

As any farther inquiry would serve only to increase the melancholy reflections which naturally intrude, when we compare the picture presented by our Hospital returns, with that of similar institutions in this or other countries, we will leave this branch of our investigation, and proceed to give the returns collected from the records kept by the Secretary of the Board of Health.

These momentous facts established by the researches of Dr. Grant ought to attract the attention of the municipal authorities of Memphis. There can be no doubt that there is a needless mortality in all great cities—a frightful waste of human life which a wise provision might prevent. We gave some details respecting the mortality of Boston in a late number. It appears from a statistical report by one of the municipal officers of New York, recently made public, that the deaths in that city during the year 1851 reached the enormous aggregate of twenty thousand and twenty-four, an increase of five thousand and forty-six, or nearly thirty per cent. over the number in 1850. The increased mortality took place chiefly among children; the increase in adults being 1,264, and in children, 3,782. The number of deaths from consumption during the year was exceedingly large, but the greatest increase was in those classes of disease which are propagated by contagion, and have their causes in the impurity of the atmosphere. The deaths by typhus fever increased from 396 to 977; by scarlet fever from 311 to 627; by dysentery from 792 to 1,113; by diarrhea from 473 to 743; and by convulsions from 1,288 to 1,792.

Compared with London, the mortality in New York is as one in thirty-one to one in forty-eight. A writer in one of the papers of that city, says there is something startling in these facts, and adds:

If there is any truth in official figures, the fact is incontrovertible that, in spite of increased wealth, improved medical skill, and the introduction and almost universal use of that invaluable blessing, the Croton water, the active causes of disease and death are increasing among us; and that the actual duration of life is yearly diminishing.

Art. VI.—*A History of Epidemic Cholera, as it appeared at the Baltimore City and County Alms-House, in the Summer of* 1849, *with some remarks on the Medical Topography and Diseases of this Region.* By Th. H. Buckler, Physician to the Baltimore City and County Alms-House. Baltimore: 1851.

This is a very carefully prepared paper, and embodies a large amount of useful information on a most interesting subject. We have only room for a few of the author's conclusions.

Theories Tested.—Before the removal of the nuisances the various cholera theories were tested as far as practicable. Saucers containing solutions of acetate of lead, nitrate of silver and other delicate reagents, were placed on the margin of the pond, and at various other points back of the north wall; and numerous strips of chemically pure paper wet with solutions of these salts were hung out at night over the different pools. Paper prepared with Schonbines' solution of iodide of potassa and starch were also used to test the presence of ozone. Duplicate experiments were instituted in the city at the same time, but without any very satisfactory results in either of the trials, the changes which occurred being nearly alike at the two places.

With the view of testing the cryptogamic and animal-

cular theories, plates of microscopic glass attached to threads by means of sealing wax were dipped in solutions of sugar, starch and gum acacia, and hung back of the north wall and in the cholera hospital. Other plates of glass were covered over with glycerine, remarkable for its property of remaining fluid for a long time when exposed to the air, and these, like the former, were suspended in various places about the establishment. Sugar and starch were selected because of the known tendency of vegetable germs to form on these compounds, and it was supposed if animalcula existed in the air that some of these would of necessity be caught on the moist and tenacious glycerine. These plates of glass, having been thus treated, were carefully examined by Dr. Christopher Johnston, aided by powerful lenses, but he was unable to detect the slightest trace of vegetable germs, animalculæ, or microscopic organisms of any sort.

It has been shown, in the last epidemic, that those who were least exposed to the miasm enjoyed the greatest immunity from cholera; that a partial removal of the fruitful sources of malaria was marked by a great reduction in the number of cases; and that with the entire restoration of the establishment, to a proper sanitary condition, the disease entirely ceased. It is fair then to conclude that for the existence of the local impurities, cholera would never have visited the alms house. But it would seem that the removal of these causes of malaria, not only prevented the further spread of cholera amongst the inmates, but what is more important, that the general health of the establishment has also been greatly improved.

If the history of epidemic cholera, as it first appeared throughout this country in 1832, be compared with the invasion of 1849, it will be remarked, as one of the most striking facts connected with this disease, that it returned to every place which it had visited during the first epidemic, unless during the interval, the locality had undergone some marked change, or entire renovation. And not only so, but the last epidemic, was far more general in its distribution than the first, many places which escaped in 1832, having been visited by the disease in 1849. Moreover, since the last invasion, cholera has shown a much greater disposition to loiter about the malarious regions; this has been the case particularly at many places along

the Ohio and Mississippi rivers. Even within the present month, two years and a half from the commencement of the last epidemic, there has been dreadful mortality from this disease on several plantations in the south-western States.

These remarks are not applicable to Kentucky or Tennessee, nor, we believe, to other western States. Many towns visited by cholera in 1833, have escaped the present visitation, though their condition in all respects save growth continues the same. The epidemic has been less general than it was in its former prevalence. Our observation has been, that the ordinary sources of malaria have hardly influenced it at all.

ART. VII.—*The Annual Address to the Graduating Class in the Medical Institution of Yale College, January* 15, 1852. By ALVAN TALCOTT, M. D., in behalf of the Board of Examiners.

This is an address full of sound and seasonable advice to a class of young gentlemen about to assume the duties of their profession. We do not know where they could go for better counsel than is comprised in the pages of this valedictory.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

Cases Illustrating the Localization of Valvular Lesions of the Heart. Reported to the Buffalo Medical Association, by AUSTIN FLINT, M. D.—The study of the auscultatory signs pertaining to heart diseases has, within a few past years, been pursued with such success, that physical exploration in this aspect, is now even more demonstrative and simple than in its application to pulmonary diseases. The physical evidences of heart lesions are, in fact, so reliable, and so readily appreciated, that, in almost every instance, the practitioner is enabled to predicate with positiveness, the existence of the latter upon the presence of the former; and, what is of hardly less consequence, he is able to pronounce with equal confidence on the absence of lesions, in cases in which he fails to discover the characteristic signs. If this remark does not apply to all the lesions to which the heart is subject, it is certainly correct with reference to those seated in the orifices, or valves of this organ. *Valvular lesions are uniformly accompanied by some of the modifications of the bellows murmur.* If there are exceptions to this rule, they are so few as scarcely to affect our dependence upon it in practice. And, on the other had, the correlative rule follows, that *if the sounds of the heart are perfectly pure, that*

is to say free from any adventitious sounds, valvular lesions do not exist.

But physical exploration carries us farther even than this. Not only does it furnish precise information respecting the presence or absence of these lesions, but it enables us to designate, in the great majority of cases, the particular orifice, or orifices at which they are situated. This minuteness of diagnosis, which to those who have devoted no attention to the subject seems almost inconceivable, and appears to be a kind of divination, in reality is attained by very simple means, which a few words will suffice to explain.

In the vast majority of cases valvular lesions are situated in the left side of the heart; that is to say, in the mitral, or aortic orifices. Why this is so, we will not stop to inquire. The fact being as it is, it follows that, for ordinary practical purposes, it suffices to determine whether the lesions are in the mitral or the aortic orifice, or in both these situations.

Lesions thus situated are, as has been stated, uniformly accompanied by a bellows sound. Now, the localization of the lesions involves certain points relating to the bellows sound. These points are two in number. The *first* point relates to the part of the chest at which the bellows sound is heard with its maximum of intensity, and the direction, from this part, in which the sound continues to be heard farthest, and most clearly. If a bellows murmur be developed at the aortic orifice, it will be heard with a maximum of intensity at a part of the chest over this orifice. This part is the intercostal space between the second and third ribs nigh to the sternum. The sound will be heard most distinctly and loudest in this spot It will continue to be heard as the instrument is carried in a direction upward from this part, the sound being transmitted by the walls of the vessels, and the current of blood expelled from the heart. It will be heard farther and louder in this, than in an opposite direction i. e. downward over the body of the organ.

On the other hand, if the bellows murmur be developed at the mitral orifice, the maximum of intensity will be found over the body of the heart, toward the part where the apex strikes against the chest between the fifth and sixth ribs. The sound produced at this orifice is more

readily transmitted to the body of the organ, toward the apex, by means of the tendinous chords and fleshy columns, than in an upward direction over the aorta above the heart; consequently, it will be obscure in the latter situation, and rapidly diminish, or disappear, as the instrument is carried above the heart, while it is distinct and loud over the part of the chest lying directly above the organ, and even below this part.

This is a point of distinction which is attended with no real difficulty in its practical application. The principle involved may be illustrated upon the healthy chest. The first sound of the heart is developed, in part, at least, at the mitral orifice. The aortic is passive in the production of this sound. The second sound is due to the semilunar valves of the aorta, and pulmonary artery. Recollecting these facts, let the stethoscope be placed over the chest between the second and third ribs near the sternum. At this point the second sound of the heart will be heard with greater loudness than over the body of the heart; and the sound will be found to be transmitted more readily above than below this spot. The reason for this is the same as in the case of an adventitious sound developed in the same situation.

But the *first* sound will be heard loudest over the body of the body and toward the apex of the heart, obeying the same law which governs the bellows murmur when produced in the same situation.

This experiment can be readily tried.

The bellows murmur, in much the larger proportion of cases, is associated with the first sound of the heart. Now the first sound accompanies the *systole*, and the second the *diastole* of the organ; hence the first is sometimes called the *systolic*, and the second the *diastolic* sound. Recollecting the relations of these two motive acts to the circulation, it will be perceived that when the bellows murmur proceeds from the aortic orifice, it is occasioned by the passage of the blood in the proper direction of the current through this artery. This is not true, however, of the bellows murmur, accompanying the systole, developed at the mitral orifice. The blood passes through this orifice from the auricle to the ventricle, and this takes place during the diastole. A systolic bellows murmur, therefore, if located at the mitral orifice, necessarily in-

volves the existence of lesions of the valves in this situation rendering the orifice patulous, and permitting regurgitation of blood from the ventricle to the auricle, when the heart contracts.

Whenever a bellows murmur accompanies the second, or diastolic sound, it must proceed either from the current of blood in its normal course from the auricule to the ventricle, or from a regurgitating current from the aorta, owing to insufficiency of the valves at this situation. In the latter case, an exception may be furnished to the rule that the sound of a bellows murmur developed at the aortic orifice is more readily transmitted in an upward direction. It is, however, extremely rare that a diastolic bellows murmur is heard; and still rarer, if indeed it be ever true, that a disastolic murmur exists without a systolic. When the former is appreciable, an adventitious sound accompanies both motive acts of the heart, constituting the *see-saw* modification.

The second point involved in the localization of valvular lesions, relates to the *pitch* of the bellows murmur. This point is omitted in some recent works treating of the physical exploration of diseases of the heart. Attention was directed to it especially by Bouillaud, in France, and by Dr. Hope. Why it is excluded from the means of diagnosis by some authors, I am at a loss to understand; for, so far as my opportunities enable me to judge, it is a point of considerable importance. Dr. Hope doubtless erred in stating that the pitch of sound was affected by distance, but that a distinction in this respect obtains between the sounds produced at the aortic and mitral orifices, is probably true of most cases; and the distinction is easily recognized, especially by one who has what is called a musical ear. The rule as laid down by Hope is, that the mitral bellows murmur has a lower pitch of sound than the aortic. Following the ingenious method suggested by Bouillaud, he represents the diatonic variation in the two sounds by comparing them to a whispered word, on the one hand, and a whispered letter on the other hand. The letter *R.* whispered, will stand as the type of an aortic bellows sound; and the word *who*, of the sound developed at the mitral orifice. On comparing the two sounds produced when this letter and word are whispered, the contrast is sufficiently apparent. These representations

are, of course, only approximations to accurrcy, and are applicable to distinguish aortic and mitral bellows sounds only when they are systolic, i. e. accompanying the first sound of the heart.

The valvular lesions are not always limited either to the aortic or mitral orifice. The valves in both orifices may be diseased in the same case. The application of the same principles, however, will in such instances enable us to determine this fact, by showing that a bellows murmur is developed in the two situations.

After this review of the diagnosis, (which has occupied more space than I had intended,) I will invite the attention of the Association to a brief report of three cases that have fallen under my observation within the last few months, in which I have been able to verify, by autopsical examinations, the correctness of the foregoing principles involved in the localization of valvular lesions. In each of the cases the diagnosis was made and recorded before the death of the patient. I have in my possession the hearts of each patient, which I shall exhibit to the Association in connection with a synopsis of the history and physical signs of the cases respectively.

Case 1.—*Disease of mitral valves, allowing regurgitation; with hypertrophy and dilatation. Autopsical appearances.*

J. D., laborer, aged twenty one. Came under observation October 17, 1851. The following is condensed from the record made at that time:—Had rheumatism when he was sixteen years of age. Several joints were affected in succession, and he went about on crutches for some time. Recollects having had pain in the chest at that time.

Five months ago first observed that in performing labor his breath was short. He was not able to do full day's work, but continued to labor more or less until the first of the present month.

It does not appear that he has had paroxysms of dyspnœa, except when occasioned by exercise, or acts of coughing.

He has had a short hacking cough most of the summer, with very little expectoration.

Appetite has been poor for the last three months. Feels worse after eating, especially after taking animal food; also after drinking spirits.

His aspect does not denote marked anxiety, or suffering. The prolabia are somewhat livid. The alæ dilate considerably with each act of inspiration. He is not emaciated, and the countenance, irrespective of lividity and dilatation of the alæ nasi, is not morbid.

Pulse is 108. Compressible, somewhat jerking.

Skin warm.

Respirations are twenty-eight, and slightly labored.

No œdema of feet or face, and no evidences of ascites.

Physical Signs.—Dullness of resonance over increased space in precordia. Considerable heaving of chest. Apex of heart strikes between the sixth and seventh ribs, about three and a half inches below the nipple, on a vertical line running through the nipple.

Above, and over most of the præcordial space the sounds of the heart are normal, *except that the first sound is not so much developed as usual. No bellows sound in these situations. At the apex a distant bellows sound is heard accompanying the first or systolic sound The pitch of the murmur is nearer* WHO *than* R, *but somewhat higher than the former. No bellows murmur appreciable except over apex of heart, and in that situation quite loud.*

In view of the foregoing physical signs, the existence of valvular disease situated in the mitral orifice, permitting regurgitation, was pronounced. The attention of the class then in attendance at the medical college was repeatedly called to the case as illustrating the physical signs denoting localization of lesions in the mitral orifice.

The patient remained under observation until his death, which occurred, January 20, 1852. During the latter part of his life he suffered from dyspnœa, requiring him to remain constantly in a sitting posture. He did not suffer from severe paroxysms of difficult breathing, or orthopnœa. The face presented marked lividity. The pulse was feeble. The impulse in the præcordia became faint and diffused. There was moderate œdema of lower extremities.

Post-mortem Examination.—The pericardial sac contained about six ounces of clear serum, with a few flocculi of fibrin. Pericardium transparent and polished. Heart greatly hypertrophied and dilated. Aortic and pulmonary valves free from disease. A few spots of atheroma in the aorta just above the valves. Tricuspid valves free from

disease. *Mitral valves thickened, presenting calcareous deposit in considerable quantity, the tendinous cords contracted, occasionally marked insufficiency.* Left auricle greatly dialted, and filled with blood partially coagulated.

Case 2.—*Disease of aortic valves. Dilatation. Autopsical appearances.*

J. Z., German, laborer, aged twenty-eight. Came under observation Oct. 28, 1851. The following is condensed from the record made Oct. 29th:—The patient states that he had good health up to two and a half years ago. He was then seized, suddenly, with severe pain in both hip joints, which continued more or less until last Christmas. About Christmas an abscess discharged on the inner part of thigh, eight or ten inches below Poupart's ligament. The discharge has continued to the present time, and still continues. He is able to use both hip joints freely.

In May last he first noticed that he was short winded. Previous to that time he was able to labor several days at a time. Shortly afterward he was obliged to give up work. He experienced at this time, occasionally, pain in the precordial region, but has had none within the last month.

He is too feeble to give a very full previous history.

He lies somnolent, in a semi erect posture, but is easily roused.

Respirations forty-eight, and labored.

Aspect notably pallid, with slight œdema of face. Feet and legs considerably œdematous.

Abdomen meteorized, without the physical evidences of ascites.

Skin cold.

Pulse 108, very small and feeble.

Urine deposits considerable albumen on testing with heat and nitric acid.

Physical Signs.—Flatness on percussion over a space in precordia five or six inches transversely across the nipple. Point of impulse of heart felt about an inch below the nipple, and to the left of a vertical line passing through the nipple. No heaving of the chest. Sounds of heart distant and confused. *A bellows murmur, high in pitch, (as high as* R,) *accompanies the first sound, heard between*

second and third ribs nigh to sternum, and not over the body or at apex of heart.

The diagnosis noted in connection with the record from which the foregoing is taken was, *lesion of aortic valves; increased effusion within pericordial sac, with dilatation.*

The attention of the medical class was called to this case as illustrating the localization of valvular lesions, by physical signs, in the aortic orifice.

Death occurred, in this case, on the 29th October.

Post-mortem Appearances.—Left ventricle and auricle filled with grumous blood, and a few white coagula. Left ventricle dilated to about twice its normal capacity. Thickness of walls but little increased. *All the valves free from disease except those at the aortic orifice, which were studded with vegetations and calcareous deposit.* Considerable atheromatous deposit in aorta above the valves. Effusion within pericardial sac estimated to be between four or five ounces.

Lungs healthy, and not greatly congested.

Liver not diminished in volume. The interlobular veins deeply congested, occasioning a granular appearance of the lobules.

Kidneys congested.

Other organs not examined.

Case 3.—*Disease of aortic valves. Great hypertrophy. Angina pectoris. Autopsical appearances.*

Mr. R. applied to me July, 1851, aged about forty. The following is condensed from the record made July 17th:—Patient was attacked three years ago with severe pain in præcordial region. He was riding at the time, and the pain obliged him to lie down at once. Had always previous enjoyed excellent health. Had never had rheumatism. Since that time he has been subject to repetitions of similar attacks of intense pain in præcordial and sternal regions, shooting down into both upper extremities. The attacks are manifestly those of angina. They occur, more or less, every week, and, of late, once, and sometimes twice daily. The pain is excruciating. It is accompanied and followed by palpitations, and by great dyspnœa. Attacks are brought on by exercise, but they frequently occur without any exciting cause. He has been unable to labor for the past three years. He is thin, but aspect not morbid. Appetite good, bowels regular.

There is no œdema. He is not habitually short-winded. His sources of complaint are the paroxysms described. If these were relieved he thinks he should be well. He has consulted several physicians of the city, and submitted to repeated bleedings with no good effect. He has also had the advice of various empirics.

A loud bellows murmur is developed in the aortic orifice, the pitch as high as R, *the maximum above the heart, accompanying the first sound.*

Pulse well developed, not irregular. He states that the pulse intermits during the paroxysms sometimes, and sometimes not.

The dorsal spine is tender on pressure.

Diagnosis. Valvular disease of aortic orifice.

The following prescription was made at this time:

℞. Ext. Hyosciam, ℥i.
Tinct. Digitalis, f ℥i.
Aquæ, ℥ii.

Teaspoonful three times daily.

Croton oil to be applied to spine.

August 11th. The following is noted:—This patient called and states that he has had no paroxysms of pain since taking the above prescription until last evening, and the suffering then was not so severe as usual. He has taken the medicine until four or five days since. It was then omitted because his supply was exhausted. Two or three days after commencing its use he called to complain of not being able to see minute objects. He was unable, for example, to see letters distinctly enough to read. The pupils were dilated. He diminished the dose of the medicine, and the affection of vision passed off in a few days. The medicine evidently produces slowness and intermittency of pulse. He has made the back sore with the croton oil.

November 10th. This patient called to see me about two weeks since, and stated that he suffered nightly from paroxysms of intolerable pain, with dyspnœa. He was sure that he could not live long, and desired that his body should be examined after death He died on the morning of this date, and the chest was examined late in the forenoon of the same day.

The heart was greatly hypertrophied, being more than double the normal size. The enlargement was most

marked in the left ventricle. The ventricular walls were but slightly increasad in thickness. Aorta enlarged for several inches above the heart, and the lining membrane opaque, and roughened by atheromatous deposit, which in several points presented calcareous degeneration. *Aortic valves thickened and much contracted.* Pulmonary valves healthy. Mitral valves slightly thickened, but not diseased sufficiently to compromise their functions.

Lungs healthy.

Other organs not examined.—*Buffalo Medical Journal.*

On Blistering in Gleet. By John L. Milton, Esq.—Not merely will a single blister frequently cure the most prolonged gleet—not merely will it rapidly sweep away all dregs of the disease in its ordinary course, but it will often cure those runnings, which have resisted all known and used methods. I have seen two blisters, with a mild injection or two, at once cure a clap which had defied the most energetic treatment; and *as I never found a case which resisted blistering and injections together, that was not complicated with strictvre, or affection of testicle,* I am slowly arriving at the conviction, *that every case of clap or gleet, however obstinate, may, if uncomplicated, be cured by blistering, singly or combined.*

To illustrate and urge forward this operation by every means in my power—to invite attention to it, that it may be put to the severe test of practice, to attest it by cases which I have collected and watched—to point out the necessity of quitting the beaten paths of treatment, and try a new remedy, more powerful than those in use, is the object of this paper. But as so many remedies have fallen into disuse from the indiscriminate use or misapplication of them, and as so many which are in favor have been arrested on their path by obstacles issuing from the same sources, I must here enter my protest against being supposed to recommend blistering in gleet, unless properly applied, and in the cases I have referred to.

In order that a blister be properly applied, there are some points, which, however trifling they may seem, require as much attention as the leading features of the case.

Where these are neglected, blistering is apt to produce such a filthy excoriated mass, that the patient will not submit to it a second time; whereas, if carefully laid on and dressed, it is, from its being out of the reach of friction, in the ordinary movements of the body, even less troublesome than if on a limb, or the trunk. Before putting it on, the hair at the root of the penis is cut off, and, if the foreskin be naturally retracted, it must be drawn a little forward over the glans. A piece of paper is then to be fitted on the penis, and cut till it exactly covers it, from the root to within half an inch of the mouth of the urethra. This is then laid down on the blister, which is cut out by it, wrapped round the penis, and fastened with threads behind the glans and near the root. The patient should remain perfectly quiet during the time it is on, lest any motion should bring the blister against the scrotum, and vesicate it; but he must not apply it on going to bed, or he will most likely fall asleep, and not awake till the penis is one mass of vesications—a state productive of an unnecessary amount of suffering.

In the milder cases, or where the skin is tender, an hour, or an hour and a half, will be sufficient. The blister is then removed; if there are any vesicated spots, they are covered with pieces of linen spread with zinc ointment, and then a layer of cotton is bound over these, and covered with a piece of linen, kept on by a thread, or, what is better, two very thin rings of vulcanized India rubber.

Where a severer case renders a more energetic employment of the remedy necessary, it must be kept on two or four hours, until free vesication is produced; zinc ointment is then applied.

To protect the penis from friction, a T bandage, with a linen bag sown into the part which receives the penis, or a handkerchief carried round the waist, and dipping in front, so as to receive the penis, and keep it up against the abdomen, is necessary.

The first effect of this application is, to increase the discharge considerably, which then terminates, either by altering its character, becoming ropy and mucous, and finally disappearing in a few days, or by remaining somewhat more persistent, and requiring a few injections, when the penis is so far advanced towards healing, that it can be handled without pain, or demanding even a second

blister. One of the most cleanly and convenient, and least painful forms of blister, is Brown's blistering tissue; it causes much less irritation, and heals much more quickly than the ordinary blister. The blistering fluids, if strong enough to vesicate, caused such pain, that I soon renounced the use of them.

How does this remedy act. By counter-irritation, will, perhaps, be the answer. But if this were the case, why should there be increased action in the urethra for a few days and why should the discharge from the urethra begin to disappear, when the counter irritant-surface is healing up? It would seem as if the organized constituents of the urethra are capable of keeping up a certain amount of over-action for an indefinite time; but that when hurried beyond this by a healthy stimulant, *a rebound* takes place, which leaves them less capable than before of furnishing a secretion, morbid in amount or in quality, or in both. We see something similar in prurigo pubis, where a blister causes an exacerbation of the symptoms, succeeded, however, in some cases, by a healthier state of the skin; in bubo, treated by blister, etc.—*Braithwaite's Retrospect.*

On the Hydrated Peroxide of Iron and Magnesia as Antidotes in Poisoning with Arsenic. By J. Haidlen.—The author has satisfied himself by experiments, that to completely precipitate 1 part of arsenious acid within five minutes by hydrated oxide of iron, 22 parts of the latter are required. Some of which had been kept for a year, had lost more than two-thirds of its power; in another case it had lost one-third of it. As is well known, on account of this property of the hydrated oxide of iron, Fuchs has recommended the mixture of hydrated peroxide of iron and magnesia, *i. e.* the mixture of a solution of persulphate of iron, which yields from 17 to 18 per cent. of the hydrated peroxide dried at 212° F., with excess of caustic magnesia. In the Wurtemberg Pharmacopœia the mixture of perchloride of iron with excess of of magnesia is prescribed. The author finds that both mixtures precipitate the arsenious acid equally as well as the pure hydrated peroxide of iron, so that there is no chemical objection to their use.—*Jahrb. für Prakt. Pharm.*, vol. xxiii. pp. 196–202.

On the Treatment of Cancer by the Lactate of Iron taken by the Mouth and injected into the Veins. By Dan'l Brainard, M. D., Professor of Surgery in Rush Medical College, &c. —About two years since I communicated to Prof. Mussey, Chairman of the Committee on Surgery of the American Medical Association, some reasons which I had for supposing that the lactate of iron was possessed of more influence over cancer than any medicine yet known.

I have, since that time, had occasion to prescribe it often with results which, while they confirm the views expressed in regard to its efficiency in checking it, have not shown that it was capable of entirely curing it. This result was to me neither surprising nor discouraging, as I have already formed and expressed the opinion that to effect a cure "the whole of the solids and fluids of the body must be brought under its influence." That this is not effected by the simple introduction of medicines into the stomach is sufficiently obvious, and indeed to be expected, since the medicine, used in that way, is subjected to the action of the same nutrition and absorption under the influence of which the disease has originated. It is necessary to go behind this; and one of the means of doing so is by injecting it into the veins. It is only recently that I have had an opportunity of putting this method to the test of practice.

Case. December 13th, 1851. Wm. H. Plumb, æt. 56 years, Englishman, applied to me on account of a tumour of the left orbit.

He gave the following history of his disease: About 25 years ago he had a disease of that eye, called by his physician cataract, which entirely destroyed the vision, but for which no operation was performed. About five years ago he received an injury of that eye from a stick striking against it, which was slight and gave but little pain. About seven months after this blow, he noticed a tumor, no larger than a pea, at the inner canthus "sending off roots into the eyeball." At this time, the tumor and eyeball were removed together by Prof. Smith, of Baltimore. The wound cicatrized well.

He remained in pretty good health about four years, when a tumor made its appearance at the lower and inner part of the orbit; which, in eight months, attained the size of a large hickory nut. It was then operated upon

again, but at the end of six weeks recommenced to grow, and, at the time of this examination, was of the size of an orange, filling up the whole of the orbit and projecting in front of it. Its surface was nodulated, elastic, pulsating, ulcerated to a great extent, and from this point there oozed a bloody serous fluid. He was thin but not sallow, and his health was not very much impaired. He complained, however, of acute lancinating pains through the orbit and head.

16th. Extirpation was performed in presence of the hospital class. It was found so firmly attached to the lower part of the orbit that it was necessary to remove the periosteum with it, and at the back part it could not all be removed. There remained a muscular mass which bled profusely, and which was so soft as to break under the forceps or tenaculum. After several ineffectual attempts to apply a ligature the actual cautery was resorted to and succeeded. The wound was dressed with lint. No inflammation followed. There was a copious discharge of red serum for a day or two, which gradually became yellow, and afterwards changed to pus. He was put, from the day of the operation, on the use of lactate of iron, gr. v, three times a day in solution.

31st. Injected into his veins f℥j of the following solution: Ferri lactis gr. viij; Aq. dest. ℥j. Carefully filter through paper.

Jan. 3, 1852. Injected ℥ij of same solution; 6th, f℥iij thrown in; 14th, ℥ijss injected; 22d, ℥ij; 26th, ℥ij; 28th, ℥ij.

Feb. 3d, ℥ij injected; 9th, ℥ijss.

During the whole of this time the wound cicatrized rapidly. At first luxuriant granulations sprang from the surface, which were repressed by the application of nit. silver. Lancinating pains continued for some time, but gradually diminished and at length subsided.

In six weeks from the operation the cicatrization was nearly complete. In eight weeks he returned home perfectly well.

The question whether the diseased mass was cancer I do not hesitate to decide in the affirmative. Its history and appearance sufficiently indicate this; its interior perfectly resembled the brain of an infant in a vascular state, and under the microscope it exhibited the most perfectly

formed cancer cells. Dr. Johnson, resident physician, fully coincided in this point.

Whether it would have cicatrized without the use of the lactate of iron cannot be determined with the same degree of certainty. Taking into consideration the return, when last extirpated, with the fact that it was afterwards impossible to remove the whole of it, I think the probability of obtaining cicatrization by ordinary means was slight. I should not, however, have thought of performing, or attempting extirpation, but that the patient (who is intelligent and trusting) expressed his desire to be submitted to the treatment when it was explained to him.

I am aware that many surgeons, under the influence of preconceived opinions, may regard such treatment as hazardous. I had fully convinced myself that such was not the case. I have repeatedly thrown gr. x lactate of iron, imperfectly dissolved in an ounce of water, into the veins of a small dog without, in any case, peculiarly bad results.

It will be seen that gr. iij was the largest quantity thrown in at a single time. It was passed in gradually and cold, and, as soon as sensible effects were produced, it was stopped. The effect noticed was a flush of the face, a fullness of the veins of the head, and a tendency to sneeze, which all passed over in a few seconds. The circulation otherwise was unaffected. If the case had not progressed favorably, and it had seemed advisable to change the nutrition more profoundly, I would have had the solution warmed and put it in slowly until its effects were perceptible. then, allowing it to pass over, have repeated it as far as appeared safe.

Up to the time of his departure the injection had been performed nine times, and grs. xix in the aggregate injected. When the activity of the salt is considered, it will be conceded that such a quantity is capable of having an effect on the system by being thrown into the blood.

In addition to that he has during this period, eight weeks, taken ʒxix of the lactate by the mouth, to what extent this may have been absorbed and carried into the circulation, or what changes it may have undergone, it is impossible to determine.

In case of a cancerous disease seated upon an extremity I should in addition to the two methods of administration resorted to in this instance, infiltrate the whole of the

diseased and the healthy tissue about it with a weak solution of the medicine. This can readily be effected by putting a ligature moderately tight about the member until it becomes œdematous, when by the aid of friction the infiltration and maceration may be effected. I had omitted to mention that all the injections were made into the veins at the elbow.

In submitting this case to the profession, I am far from claiming for it any merit which it does not possess, or drawing inferences which no single case could warrant. It is offered as an evidence of the practicability and safety of maceration through the medium of the blood systematically pursued with active substances, and to invite attention to other means of treating this inveterate disease than those which hitherto have been admitted by consent to be unsuccessful.—*Am. Jour. of Med. Science.*

Morbid Habit of Swallowing Hair; Prolonged sojourn of the foreign bodies in the gastro-intestinal canal. Evacuation of Packets of Hair by vomiting and alvine dejections. (Under the care of Dr. Crawford, Middlesex Hospital.)—Dr. Thompson has alluded, in this journal, to the case of a girl who used to swallow her hair, and had lately vomited packets of it. The patient has since then, passed per anum, a large mass of the same organic product; this circumstance induced us to inquire more minutely into the case, and we learned from the girl the following facts.

She is a servant, twenty-three years of age, now pale and thin, but formerly ruddy and stout, and was admitted Nov. 16, 1851, under the care of Dr. Crawford, with very obstinate constipation. The patient began to menstruate at the age of twelve years, and at thirteen, while in a comfortable situation, contracted the habit of picking off her hair, biting, chewing, and at last swallowing it. She went on satisfying this depraved taste for four or five months, when, being reprimanded, she gave it up, and has never resumed the custom since.

Soon after this, the patient began to feel a pain under the false ribs, on the left side, just over the spleen and the large extremity of the stomach. She was treated in various ways, and at different hospitals and dispensaries,

during several years, for this pain, no one, nor herself, suspecting that the above mentioned habit was the source of her mala ly. The general belief was, that she suffered from a tumour in the vicinity of the spleen; pain in that region, constipation of bowels, and wasting, being the principal symptoms.

At last, about a fortnight before admission, she was seized with fits of vomiting, and, among the rejected matters, a solid concretion, about the size of a walnut, was noticed; but this attracted no attention, until a second and much larger one was likewise brought up in the hospital. The nature of the affection became now apparent, but the constipation was very obstinate, and went so far as to produce stercoraceous vomiting. No more hair was noticed after these symptoms abated, until Jan. 26, about nine weeks after admission, when a very large hairy concretion was discovered in the fæces. It was of the size of the dilated rectum, measured five inches in length, and was of a deep black color. (The girl's hair is of a light tint.) The patient states that she felt this in the right iliac fossa, and she is now under the impression that more hair will be evacuated. The health has of late been rather weak, but the appetite is pretty good, and the intellect clear; but the patient complains of flatus, and of the bowels rolling in knots. This is another and very striking example of the difficulty of treating disease, when we do not know *every particular* of the history.—*London Lancet.*

Deaths from Chloroform.—In the *Boston Medical and Surgical Journal*, May 19th, Dr. A. E. Noble reports the following case:

In this instance he [the operator] gave a drachm on a handkerchief, which the patient inhaled without any apparent effect, and he then added about half a drachm, I watching the effect closely. After inhaling again a few times, she said she felt the effect a *little*, but the operator and myself could not perceive any influence. Yet willing she should think that she would not experience any pain, the instrument was applied, and as the tooth was extracted she raised her hands to her face and exclaimed "Oh!"

then letting her hands drop, she sat erect about six seconds, when her head dropped upon her breast. I felt for her pulse, but there was none. She respired not more than three or four times, and was dead! It did not exceed three minutes from the first inhalation until life was extinct. That the effect of chloroform was progressive in this case, is certain. I could detail many circumstances relating to the case, such as age, complexion, apoplectic predisposition, and other physical matters, but do not conceive it to be of interest in these days of chloroform. I will add that she was a resident of Detroit, but was in this place on a visit.

A fatal case in obstetrical practice is reported to have occurred recently in Georgia, from the same cause. And at Stamford, Conn., Mrs. Wood, wishing to have a tooth extracted, requested the dentist to administer chloroform. He complied with her wishes; after inhaling it a few times she sunk back upon the sofa and expired.

On Chloroform as an Emmenagogue. By David H. Gibson, M. D., Fort Towson, Choctaw Nation.—Having nowhere seen, in the course of my professional reading, any allusions made to the use of chloroform, as an emmenagogue, I am induced to submit the following facts for publication, partly from a desire that relief may be afforded to the suffering, and partly from a sense of professional duty.

Cases 1, 2. Occurring in the same person. In October last, Mrs. W——, having a violent headache, to obtain relief resorted to the inhalation of chloroform. Within an hour after the inhalation (which was but for a few seconds) she was flowing freely, and continued thus for *four days*. There was no irregularity of the function of menstruation in the succeeding month (Nov.), but another attack of headache supervening, she again had recourse to the chloroform, and in a half hour the menstrual secretion made its appearance, the discharge continuing for *five* days. In both instances, the chloroform was inhaled about ten days after the subsidence of her regular periods. Since the last inhalation, she has menstruated at her usual period.

Mrs. W—— is slightly inclined to plethora, general health usually good, aged thirty-five years.

Case 3. In the absence of Mrs. W——, from home, her servant girl, having gotten hold of the chloroform, imitated Mrs. W.'s example. A like result was produced upon the girl, who menstruated for four days. The girl is very healthy and about thirty years of age. The inhalation was never renewed by her. In this case, the chloroform was inhaled two weeks prior to her usual period, at which time she again menstruated. Since then she has menstruated regularly.

Case 4. Miss ——, aged 19—general health excellent —no deviation having ever taken place since her first menstrual period, was, during a visit to Mrs. ——, induced to inhale chloroform, through curiosity to experience the sensations produced by it. In a half hour the menstrual fluid made its appearance and the flow continued for four days. The inhalation in this instance was ten days antecedent to the regular period, with which it did not interfere. Mrs. W——, my informant in regard to the foregoing cases, is an intelligent and reliable lady.

Case 5. Came under my immediate observation. Was called to see Mrs. H——, found her suffering much from suppressed menstruation. To relieve urgent pain, ordered hot hip-bath, from which the patient experienced much relief. Waited three hours after the use of the bath, without recourse to any other means, having decided, as this was an opportune case, to exhibit the chloroform, which was done for *thirty* seconds. In *twenty* minutes after its administration, the patient was flowing freely and continued to do so for three days. Patient is of a weakly constitution, the result of much hardship. Age of the patient, about forty years. This case is the more remarkable from the fact that the patient has not menstruated for more than *eight* months.

The suppression was induced by causes not deemed necessary to relate at present. Prior to the suppression, she had been very regular for many years. Pregnancy has nothing to do with the case, as the patient is not at this time, nor for many years past has she been in that condition.

I regret that I have not a greater number of cases to submit for the consideration of the profession. Being but

a young practitioner, I am desirous that more experienced physicians should give the chloroform a trial, in order more fully, than my position will allow, to test its value as an emmenagogue; and diffident of my ability to account *correctly* for the "modus operandi" of the chloroform in the above cases, I shall without comment submit them to those who have better opportunities for investigation.

Before closing, however, I will present the following case. Miss ——, aged 18 years, had an acute suppression of the menses, upon the first recurrence of the monthly period, subsequent to the age of puberty. Epileptic spasms quickly succeeded the suppression. Three years have elapsed since, and she is yet subject to these spasms, at longer or shorter intervals. Within the last eight months, she was placed under my care. At one time, she will menstruate healthily—at another, there will not be the slightest appearance of menstruation—again, a leucorrhœa will take the place of the proper menstrual fluid. Sometimes, the leucorrhœal, as also the menstrual discharge, whichever it may be, will appear a week, sometimes two weeks antecedent to the regular period. Without entering into detail, as regards the treatment adopted in this case, I will submit the subjoined query. Upon the hypothesis, that the chloroform did act as an emmenagogue in the cases already related, how far would it comport with the safety of this epileptic patient to administer the chloroform? This question is predicated upon the belief, that, if the menstrual function were regularly performed, recovery from the epilepsy *might* take place. It is intended that the question shall refer more particularly to the epileptic condition, than to the irregularity of the menstrual function. I have no reason to suppose the existence of organic disease, either of the brain or uterus, in this patient.—*Philadelphia Medical Examiner.*

On the Nutrition of Muscles during their Contraction. By E. Brown-Sequard, M. D. of Paris.—It is generally admitted that when a muscle is in a state of powerful contraction, circulation, and consequently nutrition, are nearly arrested in it.

The well-known fact, that we are only able to maintain

a permanent contraction in any of our muscles for a short time only, has been explained by a loss of strength occurring, from the supposed insufficiency of their nutrition. I have frequently performed a very simple experiment which proves that the cause of the rapid diminution of the power of our will, in that case, does not exist in the muscles themselves.

The experiment referred to has sometimes been made on my legs, sometimes on my arms; and it was conducted as follows: I took a weight in one of my hands, and kept my forearm in a state of flexion, so as to form with my arm an angle of only 25 or 30 degrees. In that condition some muscles, and more particularly the biceps, were in a state of permanent contraction. My ability to maintain my fore-arm in such a position, lasted between eight and twelve minutes. When I found it was completely impossible for me to keep my fore-arm in that position, an assistant applied the wires of an electro magnetic machine to my shoulder and my fore-arm, so as to excite the biceps, and some other muscles. When, without any effort of my will, my fore-arm was maintained, nearly in the same position, during more than ten or even fifteen minutes.

After one or two minutes of galvanization, I occasionally tried again the action of the will, and I found that it was able to act anew.

If the explanation be true, that the muscle is not sufficiently nourished, and loses, in a great measure, its irritability during its contraction, and that for this reason the will becomes unable to maintain the contraction longer than a certain time, then the action of galvanism ought to be incapable of producing the contraction. If galvanism is able to act as it does, it is because the circulation of blood, the nutrition, and consequently the muscular irritability, have been very little diminished in the contracted muscles.

I have made another experiment, proving, also, that nutrition continues to take place in muscles during their contraction. If the communication established by the nerves, between the muscles of a mammal and its spinal marrow, is left entire; and if the circulation is completely stopped in that limb, by an amputation of the leg at the hip-joint, then it is found that the muscles of that leg, under the excitation of a powerful galvanic apparatus, lose their irri-

tability after ten or fifteen minutes. On the contrary, if the same galvanic excitation is applied to the muscles of the other leg of the same animal, it is found that the irritability remains a long time without a marked diminution, and that it cannot be completely exhausted. It diminishes little by little, but never disappears entirely. Therefore, it is evident that nutrition may take place in muscles during powerful contraction.

Four distinct organs are active in the case of a voluntary muscular contraction. 1st. The brain, i. e. the organ of the will; 2dly, the spinal marrow; 3dly, certain nerves; 4thly, certain muscles. Which of these is the one which is deficient in the case of my first experiment? It is generally admitted that it is the muscular irritability. My experiments prove that it is not so. Consequently, it remains to know in which of the three other organs exists the deficiency. It appears to me that it is in the brain, because a great many experiments have demonstrated that the nerves and the spinal marrow, when put strongly in action, remain very active during a long time, provided that the circulation of blood continues in them.

In saying that the action of the brain is deficient, I do not mean the action of the whole brain, but that of the part of that organ which is used in producing the contraction of these muscles, which are put in action.

From the preceding facts and reasonings I think I am justified in concluding:

1st. That it is the action of a part of the brain, and not the muscles, which is deficient, when our will is unable to maintain a permanent contraction in them.

2d. That circulation and nutrition are but little diminished in muscles strongly contracted.—*Ibid.*

A Case of Hydrophobia in the South of France.—M. Guyon, of Valence, in the South of France, has communicated to the Société d'Emulation of Paris, the report of a case of hydrophobia, drawn up in a very graphic manner, and presenting some points of interest. The patient was a nailer, about thirty years of age, who was bitten by a little dog, which he had never seen before, and which was quarrelling with his own. No signs of disease were

noticed in the strange dog; it was seen for several hours afterwards to play about, gnaw bones, &c., and was subsequently lost sight of.

The medical man who was called in wished to cauterize the three small wounds with the hot iron, but the patient refused, and his surgeon readily yielded, as the dog was supposed to be healthy. Forty days (the period vulgarly believed necessary for incubation) passed away, and the patient now exclaimed that he had been more afraid than hurt, for he had felt uneasy in his mind ever since the accident. About a fortnight after this, the man began to give signs of peevishness, and became quarrelsome and abusive. He soon complained of pain in the vicinity of the pharynx, difficulty of swallowing came on, and the fits of dyspnœa, when water was placed in his mouth, were dreadful.

The surgeon in attendance noticed that the papillæ at the base of the tongue were much enlarged, that the epiglottis became quite erect, and resembled in shape and color a small cherry. Every time that he succeeded in lowering it, the respiration became easier. There was no horror of water, but a difficulty of swallowing it, the fauces, larynx and pharynx being highly injected. The spasmodic fits succeeded each other with fearful rapidity, the cellular tissue of the neck and chest became emphysematous, and the patient died asphyxiated the day after the attack.

No post-mortem examination was allowed, but the larynx and posterior portion of the tongue were taken out; they did not present any distinct alterations. M. Guyon appends to the case the following remarks:

1st. It is clear, judging from the case just related, that a dog can communicate the disease whilst the latter is yet latent; and if it be the salivary secretion which conveys the virus, it does not seem necessary that the animal have a great abundance of it, and foam at the mouth, for the fluid to be contagious as is vulgarly supposed. 2nd. It has been imagined that fear and apprehension have much influence on the development of hydrophobia; but this case shows that the disease may manifest itself more than a fortnight *after* every sort of apprehension has passed away. 3d. The peculiar rising and stiffening of the epiglottis noticed in this case, has not been observed before, and would sufficiently explain the laryngeal spasms, which

eventually brought on asphyxia. The phenomena of hydrophobia might thus be accounted for, without having recourse to a lesion of the nervous centres, which lesion has, in fact, never been demonstrated by autopsy. 4th. The emphysema of the neck and chest was probably due to the rupture of some pulmonary vesicles during the violent spasms which the patient had experienced. This circumstance shows very clearly that there must be direct communications between the general subcutaneous cellular tissue and the submucous areolar texture of the lungs. 5th. The hypertrophied papillæ at the base of the tongue, which were seen very early in the course of the disease, would explain the sense of pharyngeal constriction of which the patient complained at the very outset. Thus it would finally appear, that it is principally in the pharynx and larynx that hydrophobia becomes localised, although it is likely that the disease arises from a virulent infection of the whole organism.—*London Lancet.*

Quinine in Continued Fever. By Wm. M. Bolling, M. D., of Montgomery, Ala.—But although I have never myself been able to cut short by the use of Quinine, an unquestionable case of Typhoid Fever, and although it is now, I believe, pretty generally the impression among such physicians of this section of Alabama as I have conversed with on the subject, that it cannot be so arrested, it is more, whatever my own belief, than I would be willing to assert, that it may not be done. I have never myself given the remedy in Typhoid Fever to the extent indeed that Dr. Fearn did, in the cases in which he succeeded with its use, though I have frequently given it in *mild* cases without this effect, in doses, with which I am in the habit daily of arresting with certainty and at once the most *violent* attacks of the various forms of miasmatic fever.—Either then my doses have been too small, or the disease now called Typhoid Fever among us, is different from the cases which were treated with Quinine successfully by Dr. Fearn, and which he calls Typhus, notwithstanding the striking resemblance between them,—and at all events, in both there is this agreement, that in their symptomology they differ widely from any of our recognized shades

of miasmatic fever. Besides the name of Typhus which he gives his cases, Dr. Fearn speaks of the disease as *continued fever*. He does not speak, however, of the post mortem appearances, and notwithstanding the resemblance, I am forced to the belief, that the disease in question was not the one now known among us as Typhoid Fever. I cannot think it possible that this malady, when established in a recognizable form, can be cut short by Quinine. With great partiality for the remedy, and not a little confidence of success, based upon a long and satisfactory use of it in many other diseases, I commenced its administration in Typhoid Fever, and was not less astonished than mortified at the result. Reflecting upon the probable cause of my failure to cut short the disease in its progress, by a remedy from which I expected so much, notwithstanding that I could temporarily control the febrile action, it occurred to me that it might be owing to an inherent tardiness of the reparative process, in the lesion of the intestinal glands. With the view, consequently, of preventing, if possible, this intestinal lesion, by arresting in its incipiency the fever, the *very early* administration of Quinine became a main point in the treatment of the disease with me, till, I think, the experiment was fairly tried.

I have given Quinine in Typhoid Fever, in its various stages, but without observing any difference in its effects, in any way connected with, or growing out of, the period to which the case had extended. I have given it in some cases as soon as the nature of the case was manifest, and n others, at various periods to the termination of the fourth, fifth or sixth week, and still the effect has been the same. I have given it, too, at so early a period, that no one, from the symptoms, could say whether the case was or was not one of Typhoid Fever, and yet the character of the case has been developed and declared after its suspension, and the disease has pursued its ordinary course. Possible exceptions have been mentioned.

As to my doses—in one case I gave twenty grains, repeating it in two hours, and thirty-six grains daily for the two or three succeeding days, in three doses of twelve grains each, at intervals of two hours; always between midnight and day, supposing that the febrile action would be less at this time. In other cases, I have given daily

say two doses of 16 grains each, or three doses of 12 grains each, at short intervals, repeated for several days in succession; and again I have kept up its continuous operation by doses of 8 or 10 grains, repeated every six or eight hours for several days; and always the result has been much the same. Nor have I neglected any adjuvant measures with which I was acquainted, calculated to secure its beneficial influence. I have given it in combination with full doses of Morphia, and I have given it without; I have preceded its administration, where the state of the bowels would admit of it, with small doses of blue mass; I have used the warm foot bath and warm drinks; I have used tepid or cold ablution of the entire surface, and cold drinks.—*N. O. Med. Jour.*

On the Respiration of the Alligator, and Mrs. Willard's Theory of the Circulation. By Albert Welles Ely, A. M., M. D., of New Orleans.—In the case of the alligator, vivisected on the 20th of August, 1851, which made Dr. Cartwright a convert to the new theory, and caused him "to become one of the standard-bearers of the *Filia nata Jovis** of the New World," we doubt whether the alligator was really asphyxiated; for the ligature was not about his trachea more than an hour. Dr. Dowler says about 30 or 40 minutes; Prof. Forshey 20 minutes; and Dr. Cartwright about an hour. Now, how is it possible that the alligator in question could have been asphyxiated, or stone dead, or dead at all, after having a ligature about his trachea for only one hour, at most; when it is well known that alligators can remain under water almost any length of time? They are known to lie at the bottom of lakes and pools of water whole days, without coming out to take the air. How, then, can an alligator be killed by tying a ligature about his trachea? Undoubtedly they become stupid on the application of the ligature, and that they remain so for a time. When they go to the bottom of lakes, they lie there perfectly still and stupid, appa-

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*Mrs. Willard has assuredly too much good sense not to disclaim this inflated compliment of Dr. C.

rently, until, after a long time, they arouse themselves and come out.

But we have proof positive on this point. We know, positively, that the ligature about an alligator's trachea for one hour only, will not kill him. I will cite a case: On the 6th of May last, the trachea of an alligator was tied by Dr. Dowler, in the presence of others of this this city, for the express purpose of repeating Dr. Cartwright's experiment of inflating the lungs, made on the 20th of May, 1851. The animal became stupid, like the other, at first; but just as they were preparing to inflate, the animal, who appeared dead, suddenly revived *of his own accord*, and fought the by-standers most desperately, *in spite of the wicked ligature about his trachea!!*

What, then, becomes of Dr. Cartwright's statement, that "it is a remarkable fact that tying the trachea is the only means by which that animal can be expeditiously killed?" And what, too, becomes of the proof, afforded by the experiment of the 20th of August, 1851, of Mrs. Willard's theory? Is it not highly probable, that that alligator was no more suffocated to death than its brother martyr to the cause of science of the 6th of May, 1852? Undoubtedly it would have revived, with the ligature about its trachea, if let alone. If Dr. Cartwright had suffered the alligator to remain undisturbed a little longer, he would have been spared the pleasure of recording the apotheosis of Mrs. Willard, and of congratulating her on the probable happy result of her new theory—the discovery of "the art, so long sought by the ancients, of making the old younger, children healthy, men vigorous, and women pretty."*

Dr. Cartwright will please excuse me for my seeming incredulity, for I am never over-disposed to take too much for granted. His Boston opponents, who probably never saw an alligator in their lives, are excusable for not questioning the data of the case.

Dr. Cartwright states, that the heart of the alligator (vivisected on the 20th of August, 1851, by Dr. Dowler,

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*See Dr. Cartwright's letter in the Boston Med. Journal of January 14, 1852.

and upon which alone he bases the "demonstration" of Mrs. Willard's theory) was dead and cold—that he took it in his hand—that not a motion or sign of life occurred. We do not question that he took the heart in his hand, and that it felt cold. He probably knows that the heart of an alligator always feels cold; but as to the heart having ceased to contract, in an alligator like that, just killed, we think there must be some mistake. The heart of the alligator is the last thing to die. It will contract itself for hours after being entirely removed from the body. Neither Dr. Dowler nor Dr. Nutt, who were present, observed that the heart was dead, although Dr. Dowler performed the dissection. These facts, together with another, that Dr. Cartwright did not write his account of the experiments to Mrs. Willard until some months had elapsed, leave room for supposing that there is some mistake about the heart being dead. We think it could not be so. The heart of an alligator, five or six feet long, is small, and contracts slowly and at considerable intervals. If the heart still contracted, in spite of the ligature about the trachea, the circulation must have been going on, and therefore the inflation of the lungs did not start the circulation.

The circulation of the alligator is independent of respiration, as is proved by its being able to remain under water for whole days, and by the experiment of the 6th of May last, in which the alligator, with his trachea tied, revived of himself, after lying stupid about half an hour. The structure, too, of the circulatory system, in the alligator, is precisely such as would lead one to suppose that he could live without breathing, and even with a ligature about his trachea. About one half of the blood in the alligator circulates constantly through his system, without ever going to the lungs at all. His structure is such, that it appears to be optional with him whether his blood be ærated or not. Like all other animals, he can live as long as his blood circulates, and this must take place, whether he is at the bottom of lakes or basking in the sun on their shores.—*N. O. Med. & Surg. Jour.*

The Cause and Prevention of Death from Chloroform.—Dr. Snow said that when dogs, cats, or rabbits were made to breathe air containing from three to five per cent of vapor of chloroform till they died—a process which occupied generally from ten to fifteen minutes—the heart continued to act for a minute or so after the breathing had ceased, as he had ascertained by means of the stethescope, and then, in some instances, the animal gave a few gasping inspirations about the time when the heart was ceasing to act, which had the effect of restoring it to life. On the other hand, when such such animals were made to breathe air containing eight per cent or more of the vapor, death took place very suddenly, the respiration and the heart's action ceasing together. He had indeed performed three experiments in which the action of the heart stopped before the breathing. In experiments with sulphuric ether, the action of the heart always survived the respiration, as air containing fifty per cent of ether vapor was not more powerful than when it contained but five per cent of vapor of chleroform. Ether could, however, be made to act directly on the heart, by continuing to exhibit it by artificial respiration after the natural breathing ceased.

He believed that no accident had occurred from the continued exhibition of chloroform vapor, well diluted with air. In the fatal cases which had happened, death had taken place suddenly, by the way of syncope, showing that the heart had been paralysed by the action of vapor constituting not less than eight or ten per cent of the air inspired just before death. He enumerated all the deaths which he considered to have been caused by the administration of chloroform. They were eighteen in number. In sixteen of these, the agent was exhibited on a handkerchief, or towel, or piece of lint, and in the two cases in which some form of inhaler was employed, it was not used by a medical man. The subjects of these accidents had enjoyed a greater amount of general health than the average of those who had taken chloroform; none of them were children or old people, and the operations which were intended or had been commenced, were, with two or three exceptions, of a trifling nature. He considered the reason of this to be, that under such circumstances the same amount of care was not always employed as in more serious cases. There were two methods of ensuring the

dilution of vapor of chloroform with atmospheric air, to such an extent that death could not occur without giving sufficient warning to allow of accidents being prevented by ordinary attention and skill.

The first and best of these methods was, to exhibit pure chloroform by means of a suitable inhaler; the other method was, to dilute the chloroform with rectified spirit of wine, before pouring it on a handkerchief or sponge. One part, by measure, of chloroform, to two of spirit, which constituted the strong chloric ether of Warren, of America, answered very well; but he (Dr. Snow) gave the preference to equal parts, by measure, of chloroform and spirit, which he was in the habit of applying by means of a sponge, in operation on the face, when he could not employ the inhaler. The best means to be employed in case of impending death from chloroform, was artificial respiration. He believed, from experiments he had performed on animals, that if it were instituted within half a minute of the apparent death of the patient, it would, in the greater number of cases, be attended with success. If this measure did not very quickly restore the patient, it would be advisable to open the external jugular vein, whilst still continuing the artificial breathing, in order to relieve the distension of the right cavities of the heart, which in these cases begins to take place as soon as its action ceases.

Dr. Crisp had found twenty cases of death from chloroform had been published. He had placed them in a tabular form, and enumerated them to the Society. His opinion respecting chloroform, deduced from those cases and his own observation was, that we could not employ it, even to healthy individuals, without some amount of danger. He differed from Dr. Snow in his opinion regarding the impossibility of robberies being effected by the agenby of chloroform, as it might easily be applied to half inebriated persons. An important question was, what are the ultimate effects of chloroform on those who take it in surgical operations? This, he contended, could only be answered by 15,000 or 20,000 cases.

Dr. Theophilus Thompson inquired the experience of Dr. Snow respecting the effects of chloroform on the system in certain cases in which the change effected was often durable or important; in fact, a train of symptoms in-

dicating more or less congestion of the brain, and lasting for many days, or even longer.

Mr. Bullock said that the effects of chloroform were materially modified by its purity; much impure chloroform was manufactured. He had seen it administered to a patient, who was three hours under its influence, without any bad effects.

Mr. Richardson believed that cases of death from chloroform had occurred which had not been alluded to by Dr. Snow, and he mentioned one in particular which had taken place in Bruges. In deaths from chloroform in animals, he found the right side of the heart congested, and left auricle contracted; for thirty-five minutes after death in one case—the peristaltic action of the bowels also continued after death.

Dr. Camps considered that the influence of idiosyncracy should not be overlooked in estimating the effects of chloroform in particular instances. He believed that the first effects of chloroform were principally on the nervous system.

Mr. Barlow thought that the danger of administering chloroform by sprinkling it on a handkerchief, had been overrated. If there were not too much chloroform used, if the handkerchief were not placed too near, and there were not too many bystanders, and the patient were properly watched, he saw no reason to fear the administration of chloroform without an instrument. Tne proper note of the effects of chloroform during its administration was most essential. One person should always be watching for these bad effects. He had never seen a patient in danger, so long as the iris remained contracted; but if the iris dilated, the inspiration became difficult and the pulse flagged, the chloroform should be immediately discontinued. The deaths in some cases, he believed, had arisen from the desire, on the part of the operator, to administer the agent too rapidly. When death occurred, he believed it was from the heart becoming suddenly affected. The history of cases, however, in which the use of chloroform had been attended with a fatal result, had not, in some cases, been sufficiently stated for us to form a correct opinion upon them; often the previous condition of the patient was not mentioned. In some of the cases better recorded, a defective condition of the structure of the

heart was present. What should we do in a case of poisoning by chloroform? We should employ all the means recommended by Dr. Snow, and more espicially resort to artificial respiration early.

Dr. Sibson said, that owing to the researches of Dr. Snow, we were now in a condition to determine the exact quantity of chloroform which was admitted into the system. He believed that in cases of death from chloroform, it was the heart that was at fault; the symptoms presenting themselves, such as sudden pallor, etc., showed this to be the case. He did not agree with Mr. Barlow, that it was necessary to stop the chloroform when the iris contracted, as in some cases, as of dislocation and hernia, it was necessary to carry the use of the agent beyond this point, which might be done with safety under proper precautions. With respect to the use of chloroform in neuralgia, it was not necessary to carry it to the extent of unconsciousness; anæsthesia was produced before this. With respect to the number of deaths which had occurred from the use of chloroform, he thought this was little, in comparison with the number of persons who had been relieved by its use.

Mr. C. Clark inquired the experience of members in respect to the employment of chloroform in midwifery.

Dr. Murphy advocated its use in midwifery in suitable cases, and with proper precautions. Deaths from chloroform were usually the result of carelessness in its administration—deaths resulting from the employment of too concentrated a dose, which acted suddenly upon the heart.

Dr. Chowne spoke at some length on the importance of the subject, and the circumspection which was necessary to be observed in the employment of chloroform. With respect to its use in midwifery, he thought many disasters had resulted from it, not only as referred to immediate, but to after consequences. Many cases had occurred which had not been published. He cautioned the members respecting the employment of chloroform, to which he was no enemy, but he was desirous to see its use accompanied with the greatest caution.

Dr. Snow said in reply, that the difference between the number of the deaths from chloroform in the list he had given, and in other lists, arose chiefly from the circumstance that he had excluded some deaths which had been

attributed to this agent, but were due, in his opinion, to other causes. The case of the child mentioned by Dr. Crisp, for instance, occurred in Germany, during the excision of a very large nævus on the side of the face and neck, which surgeons, who had seen the case previously, were afraid to meddle with. The operation lasted eighteen minutes, only nine drops of chloroform were applied altogether, and none at all during the last eight minutes. Death was evidently due to syncope from the effects of the operation. As regarded the cases in which death happened during a second attempt to render the patient insensible by chloroform, they could not be attributed to the cumulative effects of the vapor which had been inhaled during the first process; for chloroform could not accumulate for more than twenty or thirty seconds; after this time it began to be exhaled again. These cases clearly illustrated the uncertainty and irregularity of the means which had been employed in administering the chloroform, and showed that the accidents were not due to any peculiar susceptibility to its effects on the part of the patient, who could not have two different idiosyncrasies nearly at the same time; first, a want af susceptibility, and a few minutes or half an hour afterwards, a greater susceptibility than usual.

In reply to Dr. Theophilus Thompson, Dr. Snow stated, that he had not met with any unpleasant sequalæ which he considered to be the effects of chloroform, except sickness, which had in a few cases been troublesome for two or three days, and hysteria, the latter of which might certainly occur from an operation without chloroform. If depression existed from the long-continued administration of chloroform, it should be removed by warmth and cordials.—*London Lancet.*

Improvements in Modern Surgery.—Dr. Mussey in his last *Introductory Lecture*, enumerates some of the chief improvements made in surgery during the present century. He says:

In the healing of *simple wounds*, water dressings may often be so managed as to effect an entire union of the divided parts without suppuration, and this, too, where

the divided parts are not kept in contact, as is necessary for the adhesive process, the space being filled up by the requisite vital materials without suppuration. This has been called the modeling process, by Dr. Macartney.

Certain poisoned wounds: the bite of the rattlesnake, it has been alleged on pretty good authority, has been cured by the early local application of tincture of iodine.

Tetanus (traumatic): a number of well authenticated cases have been successfully treated by chloroform.

Erysipelas: locally, iodine, nitrate of silver; internally, carbonate of ammonia, opium, and quinine. In phlegmoid erysipelas, as well as carbuncle, incisions are of great value.

Anæsthtic agent: a priceless boon, recently handed down to us by a kind Providence to disarm surgery of its terrors. In my practice ether and chloroform have been used without injury, in over six hundred operations. In St. Bartholomew's Hospital, in London, according to Mr. Skey, in a statement made last summer, chloroform had been employed, without injury, in between nine thousand and ten thousand operations. The French Academy awarded for this discovery, to our countrymen, Doctors Jackson and Morton, each a medal; the one for having made the suggestion, the other for having put the suggestion to experiment.

In surgical anotomy, great process has been made. This is exemplified in the anatomy of the neck, in reference to the extraction of tumors and the ligating of vessels. The anatomy of hernia has arrived at wonderful precision, compared with what was known a century ago. Various accompaniments of strangulated hernia—difficulties to be encountered, and means of meeting them—are far more clearly understood.

Surgery of the Arteries. This department has undergone such important modern improvements, as almost to admit of its origin being dated about the time of Hunter. Ligating an artery on the cardiac side of aneurismal tumor has been chiefly practiced. The artery, too, has been successfully ligated on the distal side of the tumor, as recommended by Brasdor. Success, too, has repeatedly followed the compression of the artery on the cardiac side of the tumor; and even on the distal side, as reported by Dr. Goldsmith, of Vermont. In a recent aneurism,

according to Dr. Dudley, a compress upon the tumor, supported by a bandage upon the whole limb, has effected a cure.

In 1845, the subclavian artery had been litigated *sixty-nine times:* of this number there were thirty-six recoveries and thirty-three deaths.

Dupuytren and Liston, each has tied successfully the subclavian artery, under cover of the anterior scalenus muscle.

The aorta has been ligated *four* times, the arteria innominata *nine* times, and the subclavian, on the tracheal side of the scaleni muscles, *five* times. Death followed in every case, leaving surgeons little room to hope for any better result from a repetition of either of these operations.

The surgery of the eye has been greatly improved by Scarpa, Maunoir, Adams, Lawrence, Mackenzie, Tyrrel, Alexander, Roux, Velpeau, Sichel, Desmarses, Cuvier, Rogers, Delafield, Pancoast, Hays, and a host of others.

In the surgery of the ear, Itard, Deleau and Kramer, have been distinguished.

Plastic surgery has undergone important modern improvements. Taliacotius, of Bologna, in Italy, in the latter part of the sixteenth century, gained a temporary celebrity by his rhinoplastic operations, in which he formed a new nose by a flap of skin taken from the arm. This operation fell into disuse for a long period. Rhinoplasty, however, was revived and improved by Messrs. Carpue and Linn, of London. More recently, plastic surgery attracted the attention of numerous surgeons, as Græfe, Diefenbach, Frick, Zeis, Chelius, Delpech, Dupuytren, Lisfane, Jobert, Liston, J. M. Warren, Pancoast, Mutter, Post and Hullhen.

Staphyloraphy or palate suture was practiced with success by Roux, Græfe, Diefenbach, Warren, and others. This operation has been recently improved by Mr. Ferguson, of London, by dividing the levator palati and palato pharyngeal muscles.

In harelip operations, important improvement has been made, especially in those revolting cases with a double fissure and peninsular portion of the upper jaw connected with the septum of the nostrils, and projecting horizontally forward.

Tenotomy and Myotomy. Great progress has been

made in the relief of deformities, as of club-foot, wry neck, strabismus, permanently contracted fingers.

In chronic enlargement of the tonsils, excision is now employed instead of ligation, which was used forty years ago.

Cancer of the tongue: nearly half of that organ has been successfully removed.

Excision of the upper and lower jaw: the latter operation has been performed by disarticulating the bone without dividing the facial nerve, or the duct of Steno, thus preserving the symmetry of the face.

In chronic abscess of bone, limbs are now saved by perforating the wall of the bone, which were formerly doomed to amputation.

In fractures of the limbs, the wet bandage of Dudley, in addition to splints, should not be overlooked, as marking improvement in the treatment.

In compound fractures, the collodion is of great value in excluding the air from the wound. In un-united fracture, or false joint, the seton, sawing the ends of the bones, subcutaneous scarification, lateral or longitudinal compression of the fragments, ivory pegs and wire bridles, and more especially a suitable constitutional treatment, have, in their turn, promoted cure.

In dislocations, anæthetic agents will probably take the place of copious bleedings, nauseating doses and the extreme warm bath.

In amputations the flap operation, as well as amputations at the hip, shoulder and ankle joints, show a progress in this department of surgery.

Diseases of the joint are better understood than formerly, and the treatment is more successful. Excision of the diseased elbow joint has been repeatedly successful.

Iodine injections in hydrarthus, in enlarged bursæ mucosæ, in ganglions, in spina bifida and chronic hydrocephalus, have been practiced with important benefit.

Injuries and diseases of the brain and spinal cord are much better understood.

THE WESTERN JOURNAL.

THIRD SERIES.—VOL. X.—NO. II.

LOUISVILLE, AUGUST 1, 1852.

HEALTH OF LOUISVILLE.

The diseases of earlier summer have abated. We hear now of but few cases of bowel complaints. Bilious remittent fever is becoming somewhat prevalent in the suburbs of the city, and with it cases of intermittent fever are occurring, but in the interior nothing is heard of these diseases. Cholera is not spoken of in any part of the city.

The summer is again one of the hottest; it is indeed seldom that we have two such summers in succession. For a number of days towards the latter end of July, the thermometer ranged from 93° to 96° in a fair exposure.

CHOLERA.

This is the fourth year since cholera made its second appearance upon our continent, having broken out about the same time in New York and New Orleans, in the winter of 1848–'9. The epidemic hitherto has conformed in a remarkable manner to the course which it pursued in its former progress, and if it should continue to do so, this is the last summer that it may be expected. It appeared before in Canada, in 1832, invaded the Eastern States, and spread as far as Cincinnati, St. Louis, Louisville and Lexington in the autumn of that year. The next year its fatal ravages were felt in Kentucky and Tennessee; it lingered

until 1835, during which it visited Versailles and some other towns in Kentucky and Tennessee with great violence, and then disappeared. Many of the places which suffered in the former epidemic have thus far escaped, and some which were exempt before have suffered with the present plague. At this time we hear of its irruption in numerous localities where it was prevalent in 1833. During all the season it has appeared, from time to time, in one town and another, generally prevailing for a few days only, and with but slight mortality. The force of the pestilence would seem to be spent. It nowhere manifests the malignity which characterized its first appearance. It is not epidemic in any of our great cities, at the same time that most if not all of them have afforded occasional cases of cholera. There appears to be no tendency to an epidemic. Two or three inmates of a family are attacked and carried off, and there it stops. In this city all through the year, we have had such cases, now and then, but although dysentery has been epidemic and cholera morbus and diarrhea have been unusually prevalent, these complaints have rarely if ever run into cholera; from all which we infer that the epidemic tendency has ceased.

CIRCULAR.

We publish below the Circular of the Committee *on the Epidemics of Tennessee and Kentucky*, which we hope will awaken a due interest in the physicians of those States. The chairman of the committee informs us that although the profession was not well prepared to respond to the call of the American Medical Association when made a year ago, still a number of physicians set zealously to work and made out valuable reports. The circumstances are now more favorable. The same committee is to be continued for five years, and those who could not at once make a report will be able to do so at their leisure. But all who have any desire or ambition to aid in the advancement of their profession by investigating the epidemics passing under their observation, should go to work immediately, and collect, and arrange the statistics asked for by the committee. The enterprise is a highly important one, and it is to be expected that our brethren generally will be disposed to expend time and labor in its furtherance.

GEORGETOWN, July 26, 1852.

To the Editors of the Western Journal of Medicine and Surgery:

Gentlemen—I send herewith a call upon our brethren for materials

for compiling a history of the Epidemics of Kentucky and Tennessee. Will you be so good as to allow it a place in the forthcoming number of your Journal. I hope, too, that you will be able to allow room to the accompanying table. I look upon it as a very important item in the information sought. With all the disadvantages of last year I was very much gratified at the number of cases tabled. There were about 400 of dysentery alone. Under more favorable circumstances I hope we may do still better. I think the table at the end of each disease treated of gives a finished appearance to the job. Besides, it is an example set of giving the relative mortality of each disease, which I hope will be followed.

Very truly yours,

W. L. SUTTON.

TO THE MEDICAL PROFESSION IN TENNESSEE AND KENTUCKY.

It is known to you that the American Medical Association is anxious to be supplied with correct and reliable histories of Epidemics prevailing in different portions of the Union. For this purpose it has divided the country into districts and appointed Committees to investigate and report on the epidemics of each. To enable these committees to discharge their duties to the best advantage, they are to continue for five years.

It will be seen at once, that if the physicians of the United States will set to work, record faithfully their experience, and at the end of each year transmit to the appropriate committee the result of their observations, we shall have a history of Epidemics greatly more reliable and valuable, and embracing a range of country vastly more extensive than our profession has ever seen.

Is it necessary to argue this point? Is it needful to say that this great object can only be accomplished through contributions from many individuals? Is it requisite to say to each individual physician, that he is personally called upon to furnish any facts which may be within his knowledge in aid of this great enterprise? It is hoped not.

The committee appointed for the States of Tennessee and Kentucky earnestly desire *carefully observed facts* upon any subject connected with Epidemics; but would suggest *particular* attention to the following heads:

Causes supposed to have given rise to the Epidemic.
Causes which favored its spread.
Causes which retarded its progress.
Prophylactics.
Age, sex, color, employment, diet, and habits as to exposure and temperance of those most liable.
Time and extent of its prevalence.
Prominent symptoms during its different stages.
Proportional mortality—*Post mortem appearances.*
Treatment.

Medical topography, including the nature and geological characters of the soil—character and average temperature of the springs and wells.

Meteorological observations—mean monthly and annual temperature, weight and moisture of the atmosphere, &c., &c.

It is considered important that as many cases treated should be reduced to tabular form as possible—a form for which is annexed—and returned with the reports.

As the members of the committees are widely separated, to enable them to consult each other, it is very important that reports be made by the 15th July annually to some one of them.

Wm. L. Sutton, M. D., Georgetown, Ky.
Asbury Evans, M. D., Covington, "
Frank A. Ramsey, M. D., Knoxville, Tenn.
Thomas Lipscomb, M. D., Shelbyville, "
E. B. Haskins, M. D., Clarksville, "
} *Committee.*

Diseases.	Col'r	NO. ATTENDED.							NO. DIED.							
		Under 15		15 to 40.		Over 40.		Total	Under 15		15 to 40.		Over 40.		Total	Avr. time of Attend.
		M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F		
Cholera,	W															
	B.															
Cholera Infantum,	W.															
	B.															
Diarrhea,	W.															
	B.															
Dysentery.	W.															
	B.															
Erysipelas,	W.															
	B.															
Fever, Bilious	W.															
	B.															
Typhoid	W.															
	B.															
Scarlet,	W.															
	B.															
Hooping cough,	W.															
	B.															
Influenza,	W.															
	B.															
Measles,	W.															
	B.															
Small-pox,	W.															
	B.															
	W.															
	B.															
	W.															
	B.															

State how many of the above cases were seen secondarily,—in consultation or otherwise.

DR. GRIMES' ADDRESS.

Dr. Samuel Grimes, of Delphi, Ind., delivered an excellent address before the Indiana Medical Convention, at New Albany, in May last, which has been published. We had the pleasure of listening to the address, and have since read it with much interest. We would be glad to copy it entire, but our limits will admit of only a few extracts.

"Before we blame with asperity the wrong bias of a portion of the community towards medical heresies and quackery, we, of the profession, ought to have the consciousness that we are clearly distinguishable from the whole tribe of pretenders, by our superior attainments, not only in medicine, but in general literature and science, as well as by our greater readiness of resource at a time of difficulty and danger in the sick room. The mere fact of our holding a diploma in Latin, while others put up with one in English, and of our believing in one doctrine, while they are clamorous in favor of another, will give us no valid claim on the world for higher professional rank, and greater skill in *practice*. We must rest our claims on something more substantive and appreciable than this. We may dwell with allowable pride on the antiquity, and learning, and signal philanthropy of the great family, with Hippocrates at its head, to which we *belong*. But the pride of ancestry becomes ridiculous if it is not sustained by contemporary merit. To be received in the great brotherhood and decorated with the order of merit, we ought to be able to show that we underwent early preparation by a good academic or collegiate education; and early training afterwards, by elementary medical instruction, in the office of an experienced physician, before we were enrolled and became regular and faithful attendants in a Medical School of repute and competent organization.

"Some, perhaps many of us, will confess, with regret, that we have not been able to comply with these requisites, in all their particulars. But, surely, this does not imply that we should withhold our advice and assistance, in order to remove the obstacles from the path of those coming after us, which embarrassed and impeded our own course. The times are more propitious to the zealous student now than they were in our early days; and, consequently, there is less excuse now than then for neglect, on the part of a parent, to procure for his son a good preparatory education, and on that of a physician to impart to his pupil suitable elementary instruction in medicine. Nor need any of us stand excused from the discharge of his duty to his pupil, if he received any such in his office, by the plea of his own defective attainments. However limited may be our reading and knowledge of medical literature, we can still point out to the young student a good book on each of the different branches of medicine; and early familiarize him with the qualities of drugs, and, to a certain extent, their pharmaceutical combinations.

"In pursuing the course here recommended, we shall discharge not only our duty to our students, but, also, to the several Medical Colleges to

which they will resort after leaving us, for on the proper elementary instruction which these young men have received at our hands, will mainly depend their successful indoctrination in the higher principles and rational practice of Medicine by the professors in the schools. These gentlemen are continually embarrassed in their teachings by not knowing what foundation has been laid by the private teachers, or by those who ought to have been such; nor how far they may take for granted an acquaintance with elementary truths on the part of the youthful auditors. One professor, for fear of going beyond the comprehension of his class, contents himself with uttering the merest *common places;* another aims to bring up his hearers to his own elevated standard. If it be said that the first is the most popular lecturer, does not the fact show the unpreparedness of the students for a course of instruction such as they ought to receive, and such as an able and conscientious professor ought to be able to give.

"The dignity, learning, elevated standing, and successful teachings of the Faculties of our Medical Colleges cannot be matters of indifference to us. They are, in one sense, our representatives, in another our auxiliaries; and whatever affects their reputation must re-act on the entire body of the profession. We cannot, therefore, be supposed to look with indifference on the organization of these colleges, nor be expected to give any countenance to those with the entire competency of whose Faculties we are not fully satisfied. A new Medical School cannot, like a new store, or a new manufactory, or a new line of stages, be patronized, merely because it is new, and to encourage trade and reduce prices, by competition. Nor can personal regards nor sectional pride furnish an apology for our sending our students, or otherwise giving our countenance to a school, formed in haste out of incongruous materials, and as if in a spirit of mere business speculation. The medical profession, at large, must divide the responsibility with the Medical Schools for the attainment and fitness of the graduates sent out annually from these latter, as candidates for practice and public patronage. If we send to the Medical Colleges crude materials, it cannot be expected that they should be at once fashioned into shape and harmonious proportions. If we send to them ignorant minds, we have no right to suppose that these will be instructed and taught the principles of medical logic, and philosophy in the course of a few months.

"I repeat, that we have a large share of responsibility for the manner in which Medical education is conducted. We send our students to be educated, we influence them in their selection of the school to which they go. We, therefore, mistake greatly our position, if we think that we can assume the office of judges and critics of Medical Schools without our participating in their cares and difficulties, and giving our counsel for their protection and guidance, when they come up to what we believe to be a good standard of teaching. The best assistance we can give them, is to send them students properly prepared to avail themselves of the higher course of instruction which the professors are ready to go

through with. The best counsel we can offer to the professors, is for them to abide by their own convictions of duty in keeping up an elevated standard of medical attainments for their students, and taking adequate time for imparting their knowedge to these latter Not only are there more branches of medicine to be taught now, but each branch is much richer in details, than formerly; and hence it is clear that more time is reqeired to teach them than formerly was requisite.

"It may be asked, whether we are ourselves, either by education or subsequent reading, competent judges, in the premises, of what is necessary on the score of Medical Education? It may be that many of us are not, in this sense *competent*, but the very consciousness of deficiencies, and experience of what we have suffered in consequence may naturally impel us to seek reform, and create a desire that others coming after us should enjoy advantages of which we were deprived.

"We may, however, all of us, in this age of periodical literature and abundant publications, readily procure the desired information on many subjects of immediate interest. The calls on our time and attention in the cares of practice, and especially country practice, must always prevent our being regular and systematic readers: but if we are bent on turning all the odd hours to account, and have a good medical journal, and the best monograph at our elbow, we shall be able to keep up, to a respectable extent, with the progress of our science, at least so far as to appreciate its extent and chief bearings, and the improvements in practice. In this way we can do justice to ourselves and to our students, and confer, if need be, understandingly with our brethren, the members of the Faculties of Medical Colleges."

ENCOURAGEMENT OF QUACKERY IN GEORGIA.

The *Southern Medical and Surgical Journal* says: "Georgia is, we believe, the only State in the Union, that has drawn upon its treasury for the encouragement of quackery. The last session of her Legislature appropriated five thousand dollars to the Thompsonian or Botanic School, located at Macon."

This is humiliating. It is a premium to ignorance—an outrage upon science, and, in the estimation of educated men all over the world, disreputable to the Commonwealth. Let quackery have its course; leave it to the people, to adopt or reject it; but for statesmen—framers of laws, to go about patronizing such a system as Botanic Medicine, is simply disgraceful.

NEW MEDICAL BOOKS.

We have received a number of new medical works which we shall be able to notice at length at an early day. The publishers, Blanchard & Lea, have issued a new edition of *Dr. Bartlett's Treatise on the Fevers of the United States.* We are often asked by students of medicine for the best work on this subject, and have no hesitation in referring them to this. They will be glad to learn that a new edition has been prepared by its accomplished author, and appears in the best style of its enterprising publishers.

A new edition of *Wood's Practice* has also been issued, of which a review was prepared for the present number but was excluded by the press of matter on hand. It will appear in our next number.

Messrs. Blanchard & Lea have laid the profession under further obligations by new editions of *Meig's Treatise on Obstetrics,* and *Miller's Principles of Surgery,* both works of great ability, which have acquired a wide and enduring popularity.

The same publishers have issued the *Surgical Lectures of Bransby B. Cooper* in a style corresponding to the other publication of their house. It forms a handsome octavo volume of 759 pages, with an ample index, and may be confidently recommended to students and practitioners as a storehouse of reliable information on all Surgical topics.

Analysis of Physiology is the title of a small volume from the pen of *Dr. John J. Reese,* for which we are indebted to Messrs. Lindsay & Blakiston. It is designed to facilitate the study of this most pleasing branch of medical science, and is sure to find many purchasers among the numerous medical students who, in a few months, will be crowding the various schools of the country.

Dr. Horace Green, of New York, has made a valuable contribution to our professional literature in his little volume on the *Surgical Treatment of Polypi of the Larynx and Oedema of the Glottis,* a copy of which we have received through the publisher, G. P. Putnam, and which we shall take the earliest occasion to notice in detail.

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MEDICAL COLLEGE OF OHIO.

SESSION OF 1852–53.

The Thirty-third Annual Course of Lectures will commence on the first Monday in November, under the following arrangement, and close on the last of February:

JEDEDIAH COBB, M. D., Professor of Anatomy.
JOHN LOCKE, M. D., Professor of Chemistry and Pharmacy.
JOHN LOCKE, Jr., M. D., Adjunct Professor of Chemistry and Pharmacy.
L. M. LAWSON, M. D., Professor of Physiology and Pathology.
T. O. EDWARDS, M. D., Professor of Materia Medica and Therapeutics, and Medical Jurisprudence.
H. W. BAXLEY, M. D., Professor of Surgery and Surgical Anatomy.
LANDON C. RIVES, M. D., Professor of Obstetrics and the Diseases of Women and Children.
DANIEL DRAKE, M. D., Professor of Theory and Practice and Clinical Medicine.
WM. H. COBB, M. D. Demonstrator of Anatomy.

The Dissecting Rooms will be opened on the first of October, and continued throughout the session. Every desirable facility will be afforded for the study of Practical Anatomy.

Clinical Lectures, by the Professors of the Practice of Medicine and Surgery, will be delivered regularly throughout the session, at the Commercial Hospital.

Preliminary Lectures, during the month of October, will be delivered by members of the Faculty This course (which will be free) will embrace Clinical Lectures at the Hospital, and Lectures at the College. It will not infringe on the regular course.

The new College Building, Just finished, will be in readiness in every respect at the opening of the ensuing course; pupils, therefore, may rely on every convenience for the prosecution of their studies.

Fees—For the whole Course collectively, $105; Matriculation Ticket $5; Dissecting Ticket $10; Hospital Ticket $5; Graduation Fee $25.

Good Board (including fuel and lights) will average about 2.50 per week.

L. M. LAWSON, M. D.,
Dean of the Faculty.

Cincinnati, August, 1852.—m3

UNIVERSITY OF NASHVILLE,

MEDICAL DEPARTMENT.

The second annual course of lectures in this department will commence on the first Monday of November next, and continue till the first of the ensuing March.

Paul F. Eve, M. D., Principles and Practice of Surgery.
John M. Watson, M. D., Obstetrics and the Diseases of Women and Children.
A. H. Buchanan, M. D., Surgical and Pathological Anatomy and Physiology.
W. K. Bowling, M. D., Institutes and Practice of Medicine.
C. K. Winston, M. D., Materia Medica and Medical Jurisprudence.
Robert M. Porter, M. D., General and Special Anatomy.
J. Berrien Lindsley, M. D., Chemistry and Pharmacy.
William T. Briggs, M. D., Demonstrator of Anatomy.

The Anatomical Rooms will be opened for students on the first Monday of October.

The Students will have access to the State Hospital.

A full Preliminary Course of Lectures will be given by the Professors, commencing also on the first Monday of October.

Fee of each Professor $15. Matriculation ticket $5; Dissecting ticket $10; Graduation fee $25.

Good board can be obtained in the city at from $2 50 to $3 per week. Further information may be obtained by addressing the Dean.

J. B. LINDSLEY, M. D., Dean.

Nashville, Tenn., July, 1852—m3

DR. McMUNN'S ELIXIR OF OPIUM.

This is the Pure and Essential Extract from the Native Drug.

It contains all the valuable medicinal properties of Opium in natural combinations, to the exclusion of all its noxious, deleterious, and useless principles, upon which its bad effects depend.

It possesses all the sedative, anodyne, and antispasmodic powers of Opium,

To produce sleep and composure. To allay convulsive and spasmodic action.

To relieve pain and irritation, nervous excitement and morbid irritability of body and mind, &c

And being purified from all the noxious and deleterious elements, its operation is attended by

No sickness of the stomach, no vomiting, no costiveness, no headache,

Nor any derangement of the constitution or general health.

Hence its high superiority over Laudanum, Paregoric, Black Drop, Denarcotized Laudanum, and every other Opiate preparation.

The Elixir of Opium is also greatly superior to Morphine,

1. In its containing all the active medicinal virtues of Opium in native combination, and in being its full representative, while Morphine, being only one of its principles, cannot alone, and that in an artificial state of combination too, produce all the characteristic effects of so triumphant a remedy, when four or five of its other valuable principles are excluded.

2. In its effects, the Elixir is more characteristic, permanent, and uniform, than any of the *artificial compounds* of Morphine.

3. And as a *Preparation*, it is not liable to decompose or deteriorate like the *Solutions* of Morphine; and thus is obviated a serious objection, which has prevented the latter from being used with precision and effect.

To speak summarily, the Elixir of Opium, as a remedy, may be adopted in all cases in which either opium or its preparations are administered, with the certainty of obtaining all their salutary and happy effects, without being followed by their distressing and pernicious consequences. And in the greatest number of those cases in which no other form of Opiate can be used without occasioning the worst effects, it can also be used, with the most eminent success and the happiest results.

The discovery of this inestimable preparation, so highly purified from all the objectionable elements of the native drug, without ever changing or impairing its medicinal virtues or efficiency, is a triumph in the chemical analysis of Opium never before achieved, and places the Elixer of Opium pre-eminent in the list of all the preparations of Opium hitherto made.

Those who take Opium and its ordinary preparations cannot be ignorant of the fact that its distressing and pernicious effects result from the operation of its deleterious principles, and that the tremors, languor, and lassitude with which its devotees are afflicted, and for which they repeat the dose to relieve, are sensations of its own creation.

But in consequence of the exclusion of those deleterious principles from the Elixir of Opium, it is not liable to derange the functions of the system, nor injure the constitution and general health; hence its high superiority in all cases in which the long-continued and liberal use of Opiates is indicated and necessary to allay pain and spasmodic action, and induce sleep and composure, as in cases of fractutes, burns or scalds, cancerous ulcers, and other painful affections.

And to those persons who, from necessity or vitiated indulgence, have been accustomed to the use of Opium, this preparation will afford a gratifying substitute, by which they may relieve themselves from the thraldom of a pernicious and too frequently inveterate habit.

It possesses yet another important advantage, which is of parmount consideration in the treatment of some of the most violent and dangerous spasmodic diseases, such as Tetanus, Epilepsy, Hysteria, Tic Doloreux, Convulsions in Hydrophobia, &c., &c., which is, that the quantity necessary to overcome and control diseased action in those dreadful maladies, is not limited by the deleterious influence of those objectionable principles, but can be given in large quantities with safety, inasmuch as their proportions in Opium and its several preparations containing them, are so great, that by the time a sufficient quantity is taken for the anodyne and anti-spasmodic properties to allay and overpower the pain and spasmodic action in some of their most violent attacks, the poisonous influence of the objectionable elements will preponderate, and produce narcotism, stupor, and apoplectic death; and the patient dies the victim of the poisonous effects of the intended remedy, before the valuable properties can give the desired relief.

☞All orders from the Trade must be addressed to A. B. & D. SANDS, 100 Fulton street, New York. Sold by J. B. WILDER & CO, Louisville, Ky. FRANCIS WATTON & CO., St. Louis. And by Druggists generally.

August, 1852—6t

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J. B. WILDER & CO., Louisville, Ky.
SEATON & SHARPE, Maysville
E. B. HINMAN, Cincinnati.
FRANCIS WALTON & CO., St. Louis.

August, 1838—6t And by Druggists generally.

TRANSYLVANIA UNIVERSITY,

MEDICAL DEPARTMENT.

The 35th Session will open on the 15th March, 1853, and will continue for four months, under the same Faculty as heretofore.

The cost of the full course has been reduced to $70, *in advance;* to those who have attended two full courses elsewhere, $45. Matriculation, $5. Graduation Fee, $25. Demonstrator's Ticket, $8. Boarding from $2 to $3 per week.

ROBERT PETER, M. D., Dean, &c.

Lexington, Ky., Jan. 1853.—m3

Receipts of Medical Journal for December 1852.

Dr. C. C. Smith, Richmond, Ky,	$5 00
W. G. Williams, Chillicothe, Ohio,	5 00
J. Lundy, Salem, Ky.,	5 00
—— Hobbs, Independence, Mo.,	5 00
R. Steele, Paris, Ill.,	5 00
J. S. Halstead, Lexington, Ky.,	10 00
J. Slavens, Portland Mills, Ind.,	5 00
M. Shepherd, Payson, Ill.	5 00
P. Taylor, Rock Dale, Ky.,	2 50
C. R. McGill, Elizabethtown, Ky.,	5 00
—— Freeman, Floydsburg, Ky.,	5 00
—— Stuckey, Gosport, Ind.,	5 00
O. Jones, Brownsville, Pa.,	5 00
J. Mitchell, Steubenville, Ohio,	5 00
—— Berry, Uniontown, Ky.,	5 00
J. A. Hodgen, Woodburn, Ky.,	12 50
D. Thompson, McMillins, Texas,	5 00
J. H. Donnell, Franklin, Ind.,	5 00
M. G. Parker, New Winchester, Ind.	7 00
J. G. Price, Forks of Elkhorn, Ky.,	5 00
H. M. Ward, Columbus, Ky.	10 00
J. R. Grady, Hadensville, Ky.,	12 50
W. F. Brown, Spring Hill, Tenn.,	5 00
J. P. King, McLemoresville, Tenn.,	5 00
G. O. Pond, Griggsville, Ill.	5 00
White & Costen, Roanoke, Ala.,	10 00
A. M. Yarbro, Decaturville, Tenn.,	5 00
J. M. Botts, Frankfort, Ky.,	10 00
S. M. Linton, Columbus, Ind.	20 00

The WESTERN JOURNAL OF MEDICINE and SURGERY is published monthly by the undersigned, at the corner of Main and Fifth streets, Louisville, at $5 per annum, payable in advance. Each number contains 96 pages, making two volumes in the year of more than 550 pages each.

Contributions to its pages by the Physicians of the Valley of the Mississippi addressed to the Editors, are respectfully solicited.

Letters on business, to be addressed, postage paid, to the publishers.

PRENTICE & HENDERSON

Traveling Agents for the Western Journal of Medicine and Surgery.

C. W. James, of Cincinnati, Ohio, is our General Traveling Agent for the Western States, assisted by John T. Dent, Dr J. Wadsworth, R. K. Graves, A. R. Laws, James Rutherford, Dr. Lott Edwards Thomas Frankland and C. M. L. Wiseman.

JEFFERSON MEDICAL COLLEGE.

Session of 1852—53.

The regular Course of Lectures will commence on Monday the 11th of October, and continue until the first day of March. The ANNUAL COMMENCEMENT for conferring degrees will be held *early in March*, instead of at the end of the month, as formerly.

ROBLEY DUNGLISON, M.D., Professor of Institutes of Medicine, &c.
ROBERT M. HUSTON, M.D., Prof. of Materia Medica and Gen. Therapeutics.
JOSEPH PANCOAST, M.D., Prof. of Gen. Descriptive and Surg. Anatomy.
JOHN K. MITCHELL, M.D., Prof. of Practice of Medicine.
THOMAS D. MÜTTER, M. D., Prof. of Institutes and Practice of Surgery.
CHARLES D. MEIGS, M.D., } Prof. of Obstetrics and Diseases of Women and Children.
FRANKLIN BACHE, M. D., Prof. of Chemistry.
ELLERSLIE WALLACE, M.D., Demonstrator of Anatomy.

Every Wednesday and Saturday in the month of October, and during the Course, Medical and Surgical cases will be investigated, prescribed for, and lectured on before the class. During the past year *two thousand and twenty-nine* cases were treated, and *three hundred and eighty* operations performed. Amongst these were many major operations,—as amputation of the leg, thigh, arm, hand, mammæ, &c., trephining, tying the carotid, extensive plastic operations, resection of the femur for ankylosis, removal of the superior and of the inferior maxillary bone, reduction of dislocations—some of old standing, treatment of fractures of the thigh, arm, forearm, &c., &c.

The Lectures are so arranged as to permit the student to attend the Lectures and Clinical demonstrations at the Pennsylvania Hospital.

On and after the 1st of October, the dissecting rooms will be open, under the direction of the Professor of Anatomy and the Demonstrator.

FEES.—Matriculation, which is paid only once,..... $ 5
Each Professor, $15,..... 105
Graduation,..... 30

The number of Students during the last Session was 506; and of Graduates 228.

R. M. HUSTON, M. D.,
Dean of the Faculty, No. 1 Girard Street.

August, 1852.—1m

MEMPHIS MEDICAL COLLEGE.

SESSION OF 1852-'53.

The regular course of Lectures will commence on the first Monday in November, and continue four months.

AYRES P. MERRILL, M. D., Prof. of Materia Medica and Therapeutics.
LEWIS SHANKS, M. D., Professor of Obstetrics and Diseases of Women and Children.
EDWARD H. LEFFINGWELL, M. D., Prof. of Chemistry and Toxicology.
HARDY V. WOOTEN, M. D., Prof. of Principles and Practice of Medicine.
HOWELL R. ROBARDS, M. D., Professor of Surgery.
ARTHUR K. TAYLOR, M. D., Professor of Anatomy.
CHARLES T. QUISSTARD, M. D., Prof. of Physiology and Pathology.
DANIEL F. WRIGHT, M. D. Demonstrator of Anatomy.

The Class will have free access to the Hospital during the Session, when, in addition to the instruction given by the Surgeon, Dr. George A. Smith, there will be regular Pathological and Clinical Lectures by the Professors.

The Lecture, Anatomical, and all other necessary rooms in the College are large and comfortable; and arrangements are made to supply the school, by the next session, from the collections of Europe and this country with full Chemical Apparatus, Anatomical Museum, and every thing required to demonstrate the different branches. The material for the practical study of Anatomy is abundant, and special attention to that branch will be given.

LEWIS SHANKS, M. D.,
Dean of the Faculty.

May, 1852—m7

UNIVERSITY OF LOUISVILLE,

MEDICAL DEPARTMENT.

The Lectures in this Department will commence on the first day of November next and terminate on the last of February.

CHARLES W. SHORT, M. D., Emeritus Professor of Materia Medica and Medical Botany.

BENJAMIN R. PALMER, M. D., Professor of Descriptive and Surgical Anatomy.

LUNSFORD P. YANDELL, M. D., Professor of Physiology and Pathological Anatomy.

SAMUEL D. GROSS, M. D., Professor of the Principles and Practice of Surgery.

HENRY MILLER, M. D., Professor of Obstetric Medicine.

LEWIS ROGERS, M. D., Professor of Materia Medica and Therapeutics.

BENJAMIN SILLIMAN, Jr., M. D., Professor of Medical Chemistry and Toxicology.

AUSTIN FLINT, M. D., Professor of the Theory and Practice of Medicine.

T. G. RICHARDSON, M. D., Demonstrator of Anatomy and Director in Pathological Anatomy.

The fee for admittance to the Lectures of each Professor is $15, payable invariably in advance. Matriculation and Library fee together, $5. Graduation fee, $25. Practical Anatomy and Dissection, $10—ticket to be taken at least once before graduation. Rooms open from 1st October.

A preliminary course of lectures, free to all students, will be delivered during the month of October.

Clinical instruction is given twice a week at the Louisville Marine Hospital. Ticket $5, to be taken once before Graduation.

A Clinique has also been established in connection with the University, at which operations are performed and cases prescribed for and lectured upon in presence of the class.

☞Good boarding can be procured at from $2 50 to $3 a week.

L. P. YANDELL, M. D.,
Dean of the Faculty.

July, 1852.—4m

STARLING MEDICAL COLLEGE.

COLUMBUS, OHIO.

The Regular Course of Lectures in this Institution will commence on Monday, the first day of November next, (November 1, 1852) and continue four months. The *preliminary course* will commence on Monday, the 18th of October, and continue to the end of the month.

During the latter course, the following subjects will be taught:

Minor Surgery	Dr. Howard.
Insanity	" Smith.
General Physiology	" Carter.
Microscopical Anatomy	" Paddock.
Poisons	" Hills.

The above course is free to all matriculants.

FACULTY.

R. H. PADDOCK, M. D., Professor of Anatomy and Physiology.
RICHARD L. HOWARD, M. D., Professor of Surgery.
SAMUEL M. SMITH, M. D., Professor of the Practice of Medicine.
FRANCIS CARTER, M. D., Professor of Obstetrics and Pathology.
CHARLES A. LEE, M. D., Professor of Materia Medica, and Medical Jurisprudence.
FREDERICK MERRICK, M. D., Professor of Chemistry.
C. C. PARKER, M. D., Demonstrator of Anatomy.

FEES.

Lectures	$60 00	Graduation	$20 00
Matriculation	3 00	Dissecting Ticket	5 00

Weekly Clinical Lectures will be delivered, and Surgical Operations performed before the class, upon cases derived from the College Hospital and from Private Practice.

Letters addressed to the Dean, or any Member of the Faculty, will be promptly answered.

S. M. SMITH,
Dean of the Faculty.

July, 1852.—2m